[COMMITTEE PRINT]

SELECTED READINGS ON RESEARCH AND DEVELOPMENT EXPENDITURES AND THE NATIONAL ECONOMY

PREPARED BY THE

SUBCOMMITTEE ON
DOMESTIC AND INTERNATIONAL
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LETTER OF TRANSMITTAL

HOUSE OF REPRESENTATIVES,
COMMITTEE ON SCIENCE AND TECHNOLOGY,
Washington, D.C., April 1, 1976.

Hon. OLIN E. TEAGUE,
Chairman, Committee on Science and Technology,
House of Representatives, Washington, D.C.

DEAR MR. CHAIRMAN: I am transmitting herewith a set of selected readings on the subject "Research and Development Expenditures and the National Economy." These readings are intended to provide a detailed background for our Subcommittee's forthcoming hearings on this subject.

The Federal Government is currently spending almost $25 billion each year for a wide range of research and development activities. The private sector spends approximately $23 billion for the same purpose. This vast enterprise, covering activities from basic research to specific development projects, impacts in numerous ways on our national economy, and is, in turn, impacted by developments in the national economy.

In developing this Committee Print, the staff was assisted by the Science Policy Research Division of the Congressional Research Service, the Innovation Information Center of the Program of Policy Studies in Science and Technology at the George Washington University, and the Division of Public Sector Programs of the American Association for the Advancement of Science.

I believe that these materials will provide a well rounded background for our hearings, and I commend them to you and the members of the Committee on Science and Technology.

Sincerely yours.

RAY THORNTON,
Chairman, Subcommittee on Domestic and International Scientific Planning and Analysis.

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INTRODUCTION

Selected Editorials, Articles, and Excerpts From Texts on Research and Development Expenditures and the National Economy

There is a vast literature on research and development expenditures and their impact on the national economy and, conversely, the developments in the national economy which impact on research and development. The reading materials included here are divided into the following separate sections depending on their primary focus: impact of R&D expenditures on the national economy; issues surrounding Federal R&D expenditures; effect of R&D in private industry; and, international aspects of United States R&D Funding.

This division of the materials is not to suggest that the issues do not overlap. In fact, in many instances placement seemed almost arbitrary and was determined by the primary focus of the author. There was also an effort to introduce the reader to varying views on each issue.

The context for the effort of the Subcommittee on Domestic and International Scientific Planning and Analysis of the Committee on Science and Technology to address the wide ranging issues surrounding R. & D. expenditures and the National Economy was stated by the committee's Chairman, Hon. Olin E. Teague (D.-Texas), in a statement on the proposed National Science Policy and Organization Act of 1975 (Committee Print, 94th Congress, First Session, Serial C). Mr. Teague observed that of the many reasons for the legislation under consideration, the issues "cardinal" to the efforts of the committee were that:

> (1) We recognize the prominent role which applied science has played in producing the great problems of modern civilization—the crowding and congestion, the excessive gobbling of natural resources, the dangerously shifting foundations undergirding the economy, the disruptive social and moral influences abroad in the land, and so on. Indeed, such recognition was directly responsible, and in large measure, for the concept of Technology Assessment and the formation of the legislative Office which now bears that name. We know the need to understand as best we can *all* the probable impacts of technologies as they develop—good and bad.
>
> (2) We are further aware, particularly as we look about and see the critical problems facing us with regard to food, energy, materials, security, economic strength and the like, that the solutions to our problems depend in some way upon the judicious use of better technology. Former Presidential science adviser Dr. Edward E. David has put is succinctly:

> Can we be sure that science and technology will find the answers? Can we be sure that solutions to our problems exist? No, but we can be sure that nothing but science and technology can find them if they do exist. To put it as bluntly as possible: science and technology must answer our problems. If they don't nothing else will.

This may be overstated, but its germaneness to the needs of our era has been recognized, openly or tacitly, by every Administration of the past 45 years.

The Subcommittee is assessing the impact of R. & D. expenditures and changing strategies at the Federal level on the national economy. As the Subcommittee Chairman stated in correspondence with Subcommittee members regarding the rationale for hearings on this timely topic, "The President's budget proposal for fiscal year 1977 includes $24.7 billion, an increase of 11 percent over the current year. While this increase comes at a time of severe budget constraints, it also comes at a time when the U.S. R. & D. effort is feeling the pressure of several external factors. Among these are the effects of inflation, the strong progress of R. & D. in other advanced, industrial nations, and the feeling in industry that increased regulation is causing a diversion of industrial R. & D. funds into 'preventive' rather than 'innovative' research. The hearings on this subject will seek to determine the rationale for the new budget, the priorities within it, and the effects which these priorities can be expected to have both on science and technology and on the economy generally."

Section I—Impact of R&D Expenditures on the National Economy

"Science for Economic Growth and Social Change"

By Jacob E. Goldman

From *The Interaction of Science and Technology,* W. Dale Compton, ed. Urbana, Illinois: University of Illinois Press, 1967, p. 9.

First came the ivory tower, then the corridors of business and commerce, then government and the apparatus of war and now—with sudden impact—society at large. This has been the progression of acceptability and utilization of scientific research.

Until relatively recently, scientific research (as distinct from technological or engineering research) was the exclusive province of the academic campus. Its utility was considered at best vague and the thread that linked its practice to the evolution of new technologies so winding—both spatially and temporally—that no one felt any compulsion to intrude upon the privacy and sanctity of the researcher. Research was (and is) synonymous with acquisition of new knowledge. Where but at a university should such aspirations find a home?

Then came the period between the two great wars—the period of great industrial expansion in the United States. Here and there it was discovered that science can be profitable in leading to and pointing up new materials, methods, and devices and thus bridge the gap—both in time and in space—between scientific discoverey and technological invention. Research suddenly became a corporate asset—an intangible one, but of sufficient reality to be recognized by the more progressive and aggressive corporate managements.

During World War II and its aftermath, the government joined the parade. It realized that science can be not only an industrial asset generating economic opportunties but an essential asset in maintaining the security of the nation. Military weaponry and national prestige—particularly in space exploration—grew to depend more and more upon the nation's scientific capability. Government suddenly realized its responsibility not only to exploit the fruits of science in these areas of national interest but to assure the maintenance of the capability—survival of the species. Government thus became the principal supporter of science and of the scientists who reside in their native habitat of the campus, in the more recently acquired environment of industry and in the inhouse apparatus created by the government for this specific purpose.

The impact of the wartime and postwar triumphs of science has more recently brought a new dimension into this saga of the care and feeding of science and the scientist. The bomb, radar, the conquest of space and the emergence of the computer have made it clear that science touches upon the everyday life of every man. Aided and abetted by communications media, concern for science in all its manifestations

has spread to the public at large. Science is written about daily in the public press; there are even several comic strips devoted to the serious expostulation of matters scientific and some not-so-serious ones that will tell you that whoever masters magnetism will rule the world; it is talked about daily in the legislative bodies at national, state, and local levels; it has even conferred a sort of prestigious status on its practitioners. The scientist in the public eye is no longer the mad iconolast but a normal human being.

But this spreading of the informational and financial support base has brought with it a whole new series of problems and challenges. It is to some of these that I chose to devote the balance of my remarks today.

A question being asked again and again these days is: Why can't the science that created nuclear energy, that put a man in space, that fashioned incredibly fast and powerful computers—why can't this scientific apparatus be put to work to solve our major social problems? If, indeed, the techniques of science are so powerful, so all embracing—goes the query—why can't they be put to work improving the lot of man—putting an end to the major social difficulties confronting us? All we have to do is to institutionalize the process, a la the Manhattan project, and all the ills of the world will disappear through the magic of science.

* * * * * * *

I would like to return now to the field of transportation. Wherein, you might ask, lies the scientific component of transportation. Unlike, say, communications or ordnance, there are no obvious underlying disciplines which can combine to advance well defined frontiers. Because of our intense preoccupation with problems of transportation—we are in the transportation business not just the automobile business—we asked ourselves the same question. To find some answers, we put together a small group—a physicist turned operations researcher, an electrical engineer, and others—to see what dimensions can be used to describe and understand transportation. We have learned a few interesting things one of which I should like to describe for you.

I believe that we all recognize intuitively that transportation affects the way in which a region develops: that transportation is a part of the urban system incapable of isolated analysis. Further complicating this analytical problem is the fact that we are confronted with a profusion of technology—hardware ranging from conveyor systems to VSTOL aircraft—with little quantitative knowledge to guide its application.

One of the first questions which we asked ourselves was are there any mathematical tools or analogics which would enable one to quantify and optimize transportation relationships—somewhat in the manner of the use of Monte Carlo methods in the analysis of communications networks. Clearly, the philosophy of systems analysis was indicated, and the lack of an exact solution led to consideration of mathematical modeling and simulation as a means of analysis.

Consider the question of the growth of a city under the influence of population, economics, industry, transportation, topography, and other constraints on land use. It is possible to develop a mathematical model which, based on projections of basic industrial activity, will

"move" a region through time, locating new households and commercial activity in accordance with a set of rules concerning accessibility and other factors. We have developed such models and tested them in real world-cities by running them backwards. Once calibrated, such a model can assist the planner in determining the consequences of a decision on, for example, the location of a transportation link. A serious communiication problem arises, however, when the planner is faced with a computer output consisting of thousands of population figures in tabular form, and when he must translate his proposed plans into a complicated coordinate system for computer input.

We have made a small stride in the direction of better planner-simulation communications with the cooperation of the Coordinated Sciences Laboratory of the University of Illinois, whose facilities we are dedicating today. A film was generated here that illustrates a hypothetical region which is to be served by a new road. The planner can study the effect of alternate road locations by drawing the proposed route on the face of a scope with a light pen and watching the results of the simulation as they are plotted by the computer.

I would be hesitant to attempt to extrapolate these relatively limited experiences to the exhaustive solution of the many social ills of the world. It does seem to me, however, that one of the formidable obstacles to be overcome is an institutional one. The science and the scientists that will help solve these problems will still be concentrated largely on academic campuses and will continue to maintain their primary loyalty to their disciplines, their professional journals, and peer groups. Such institutions as the Coordinated Science Laboratory are perhaps a step in the right direction, in that they provide a focus for the articulation of needs that transcend the traditional boundaries of the intellectual marketplace. Weinberg sees in his new and urgent social demand a possible role for the National Laboratories. But more than anything else, we need a cadre of scientifically trained accomplished manpower who see a *professional* goal in ministering to the needs of a world that transcends the four walls of their disciplinary prison.

Beyond the needed modification in the institutionalization of scientific research and facilities lie two additional levels where new approaches are called for: methodology and support.

New scientific methods and techniques have emerged to help the physical scientist advance his frontiers. Many of these, such as the new solid state electroncis and computer technology, were born and bred by military support. The application of science to social problems requires an exploration for useful methodologies that may already exist or may lie hidden in the rich storehouse of emerging knowledge from the behemoth of presently supported research. One technique, systems analysis, which is showing promise in attacking these complex social problems, is also a direct result of a wartime need. Formulation and quantification of goals—often conflicting, occasionally contradictory—and optimum deployment of limited resources to approach these goals is as much a concern of the city planner or public health officer as it is the concern of the military commander. Methods developed by mathematicians for evaluation of weapons systems effectiveness may, for example, provide the only practical way of comparing the probable consequences of alternative solutions to social problems.

Finally, the scientific community and the government community must reexamine the support base for research. Nearly all present-day basic science derives its support from the military and space establishments, from the AEC and from the National Science Foundation. The scientific community must now face squarely the challenge to press other segments of the government and society to support science if they are to expect to derive benefit from it. The challenge to government is equally sobering. Those sectors of government more directly concerned with the problems of social change, e.g., HUD, DOT, HEW, Commerce, must accept a share of the responsibility for the support of science. The very arguments advanced to create the military support agencies for basic and academic science are just as valid in arguing for support of science by these other agencies. If they expect the scientific community to generate interest and concern for the problem lying within their purview, they must provide the principal catalyst for coupling science and scientists to their needs. Financial support is the best catalyst I know for generating this coupling. Maybe what I'm asking for is a social ARPA or an ONR for HUD and an OSR for DOT. However it is accomplished, the precedent has been established by these foresighted mission-oriented military agencies. It remains for the nonmilitary ones to profit from the national experience.

"U.S. Government Support for Civilian Technology: Economic Theory Versus Political Practice"

By George Eads

Research Policy, 1974, volume 3, p. 2.

During the last two years the U.S. Government has undertaken a new series of initiatives designed to stimulate increased commercialization of advanced technology with the twin aims of improving domestic efficiency and enhancing international competitiveness. These new initiatives have been justified, at least in part, by an appeal to an economic concept known as externalities. Perhaps the most concise statement of the basic theory of externalities and its policy implications for U.S. Government involvement in private R. & D. investment decisions was contained in the 1972 Economic Report of the President's Council of Economic Advisors [1]:

"* * * Government has an appropriate role in R. & D. even when its results will not be incorporated in Government purchases, because private firms would underinvest in R. & D. for goods normally purchased by the *private* sector. Although an investment in R. & D. may produce benefits exceeding its costs from the viewpoint of society as a whole, a firm considering the investment may not be able to translate enough of these benefits into profits on its own products to justify the investment. This is because the knowledge which is the main product of R. & D. can usually be readily acquired by others who will compete away at least part of the benefits from the original developer. This is particularly true of basic research, where the output frequently occurs in the first instance not as a marketable product, but rather as an advance in basic knowledge that can subsequently be used in

applied research and development by a wide and often unforeseeable range of firms."

Language similar in tone also appeared in the President's Message on Science and Technology, delivered March 16, 1972, in which the nature of the proposed U.S. Government initiatives was detailed. Such statements ought to bring joy to the hearts of economists interested in the rationalization of U.S. Government support for commercially oriented science and technology. They contain an explicit recognition that the principal justification for such support is the failure of markets to perform their job adequately. More significantly, implicit in the President's message is a commitment that the new programs he was announcing were designed to supplement, not supplant, the market. Their aim was to create a set of institutions to diagnose and correct market failures so that the market then could be relied upon to provide correct signals for private investment in technological change.

However, any such joy ought to be tempered with anxiety as to what is likely to be the result once these admittedly admirable intentions begin to be implemented. Indeed, there is ample precedent for concern. The economic history of the United States is full of attempts by the government to correct through direct intervention what have been perceived by some as failures of markets to direct economic activity properly. The regulation of the railroads is an early example. The decision to undertake economy-wide wage and price controls is a more recent one. However, in an unfortunately large number of cases, these attempts have been unsuccessful. The market failures, either real or imagined, have not been corrected, and, what is worse, a host of new distortions have been created. In certain cases the performance of industries subject to direct economic regulation has been so dismal that legislation has been proposed that would substantially decrease the scope of regulation and increase the reliance upon markets, imperfect though they may be. (The Regulatory Modernization Act, introduced in the fall of 1971 by the Nixon Administration, which proposed relaxation of rate regulation and freer entry for surface freight transportation is one example. The deregulation of natural gas prices, proposed by the Administration in 1973, is another.)

Therefore, though I am encouraged by the fact that the general nature of the market failure justifying government intervention has been correctly perceived, and though I am heartened by the stated resolve of the current administration to employ solutions appropriate to the problem, I am extremely apprehensive about what is likely to be the outcome when government bureaucrats, operating in a highly charged political atmosphere, attempt to improve upon the workings of impersonal markets. My apprehension is heightened by the way I see actual U.S. Government science and technology policy developing. Already there is a large gap between the admirable stated intentions and actual practice. This gap is caused in part by the fact that the theory of externalities and the conditions under which its simplest prediction is a proper guide to policy have not been clearly understood by those formulating U.S. Government science and technology policy. This misunderstanding has been abetted by the failure of economists to present the theory of externalities in an operational form.

We economists have given policy makers a theory that possesses a great deal of political attractiveness, but we have failed to develop the tools that would allow us either to show those governmental officials charged with implementing science policy how the theory should be applied in specific cases or to demonstrate to them and to the public that the theory is being misapplied. This is not to say that economists are powerless to point out gross misapplications of the theory. This can, and obviously should, be done. In the most cases, however, the proper policies may not be obvious even to economists, given our current state of understanding. Therefore economists should not be too quick to criticize when policy makers, confronted with an attractive theory and faced with the political imperative of doing something, proceed to produce what may look like bad policy.

Let us focus on why the simplest predictions of the theory of externalities may prove an inaccurate guide for the formulation of technology policy in specific industries. The basic assumption underlying the prediction of a tendency towards underinvestment in technological change by private industry is that currently decisions are being made in an otherwise neutral atmosphere. That is, it is assumed that the absence of a mechanism designed to allow capture of externalities is the only market-distorting force in operation. This assumption clearly is violated in practice. For example, in certain oligopolistic markets, by tacit agreement of the firms involved, competition has been directed away from price and towards product improvement. The firm that has been able to offer a slightly improved product has been able to gain a substantial edge over its rivals. This has meant that to survive in such industries, firms have been required to devote a share of the resources that otherwise might have represented profits accruing to their market position to research, development, and new product design. Prices have been held higher than they would have been under more competitive market conditions, and the rate of technological change in the industry has been increased. In certain cases the sum of investment in technological change by individual firms responding to such incentives may already exceed the socially optimal level. Or to state the result in more technical language, due to the peculiarities of market structure and product characteristics which channel competition into nonprice areas and which channel nonprice competition into competitions for product improvement, the private rate of return to innovation may in some cases exceed the social rate of return, leading to an *overcommitment* of resources to the process of technological change. (This point was suggested by Harvey Brooks, who singled out the pharmaceutical industry as an example where an overcommitment of resources to technological change may have occurred as a result of this phenomenon. An article by Comanor [2] suggests that the tendency toward higher levels of investment in technological change for competitive reasons exists throughout the consumer durable and investment goods sectors. The argument presented here is nothing more than a variant of the well-known proposition that the amount of nonprice competition in oligopolistic markets is likely to be higher than socially optimal. Ordinarily the only type of nonprice competition considered is advertising, yet new product development may be an equally expensive and, in some cases, more competitively effective means of nonprice competition.)

Another factor influencing the rate and direction of private investment in technological change is the substantial U.S. Government influence, both direct and indirect, that already exists. Since the early 1950's the U.S. Government has consistently provided more than half of the funds going to support research and development, though to be sure, support has been concentrated in a very few sectors, primarily those concerned with defense and space technology [3]. This funding has produced the knowledge base for many of our most technologically advanced industries. As important as this direct influence has been, however, the indirect governmental influence on technological change may have been even greater—particularly in the majority of industries which were not among the beneficiaries of this largesse. Among such indirect influences have been the following:

The U.S. Government is a major purchaser of goods and services; even in some non-defense industries it may represent the largest single purchaser. (For a discussion of the effects of U.S. Government procurement on the incentives to innovate and how these effects might, in the view of the authors, be profitably redirected, see Nelson et al. [4].)

U.S. Government programs which are designed to promote broad social aims, such as Federal Housing Administration loan guarantees. Rural Electrification, Medicare and Medicaid, grants to local communities for improved police protection stimulate demand in these areas.

The national, state, and local governments subject major sectors of the economy to direct economic regulation. (Scherer estimates that in 1965 the industries subject to direct economic regulation contributed 10.9% of the nation's GNP [5].

Industry in general is subject to the influence of the tax laws, the regulations of the Environmental Protection agency, the anti-trust laws, and the patent laws.

Wherever these regulatory or quasi-regulatory interventions exist, there is likely to be some impact on the cost and benefits of the innovation as perceived by private parties and, hence, some influence on the tendency towards underinvestment as predicted by the theory of externalities as simply expressed. In certain industries the net influence of current governmental actions may have been to accentuate the tendency towards underinvestment. In others, existing U.S. Government policies, taken together, may have already overcompensated for any tendency towards underinvestment, so that programs designed to simulate the results that would be produced if externalities could be fully taken into account might well call for the institution of mechanisms designed to *discourage* additional private investment in technological change.

What is readily apparent is that the appropriate degree of compensation for any tendency to underinvest will vary widely by industry. This suggests that industry-specific programs are more likely to produce appropriate results than are programs applicable to all industries. Also implied is that a prerequisite to the intelligent formulation of science and technology policy by the U.S. Government is a clearer understanding than currently exists of the net impact of the mass of regulatory, tax, patent, anti-trust, procurement, and trade policies (just to name the most obvious) on private incentives to invest in technological change. In most cases any such linkages that exist

are currently only dimly perceived even by the agencies charged with administering the policies mentioned. They are all but unknown to those agencies charged with the formulation of U.S. Government science and technology policy.

Some means must be found to correct this situation. Perhaps consideration should be given to requiring that U.S. Government agencies charged with regulating various segments of the economy be required to produce "Technology Impact Statements", analogs of the Environmental Impact Statements that currently must be prepared to accompany their major policy decisions. Certainly it makes as much sense for the Interstate Commerce Commission, the agency charged with the regulation of U.S. rail and motor carriers, to consider the effect of its rate and entry policies on the railroads' incentive to innovate as it does to consider the impact of these same policies on the environment. (Any attempt to do just this is contained in the document *Improving Railroad Productivity; Final Report of the Task Force on Railroad Productivity*, A Report to the National Commission on Productivity and the Council of Economic Advisors (November 1973).) Indeed, a major candidate for preparing Technology Impact Statements would be the Environmental Protection Agency itself. Many of this agency's actions have a major (if currently unrecognized or unacknowledged) influence on the R. & D. decisions of many industries. (See. for example, *Cumulative Regulatory Effects on the Cost of Automotive Transportation*, Final Report of the Ad Hoc Committee, prepared for the Office of Science and Technology (28 February 1972).)

Admittedly, it might prove administratively infeasible to require Technology Impact Statements even if only for major decisions. However. some means needs to be found to raise the "innovation consciousness" of the governmental agencies involved in direct and indirect regulation and to provide higher level decision makers with information about the current state of incentives so that any programs they propose to institute to correct "market failures" with regard to private investment in science and technology will be likely to produce appropriate results.

To illustrate the scope of such influence, I have compiled the following list of U.S. Government policies which have a significant impact on the rate or direction of technological change in the commercial aircraft industry. By this choice I do not mean to contend that this industry is typical as far as the degree of U.S. Government influence on its pattern of technological change is concerned. It is, in fact, perhaps the leading example of such influence. Its choice may be justified on the grounds that even in what is perhaps this most obvious of cases, the degree of this influence is not at all understood by the relevant decision makers, as certain policy proposals discussed below will illustrate. The list probably is not exhaustive. In particular, no attempt has been made to include these policies which have an impact on all industries such as the favourable tax treatment accorded to R. & D. expenditures. Finally, no quantitative attempt has been made to compare the level of technological change that actually exists in this industry with whatever externalities may exist, though some quantitative estimates of impacts would be a necessary part of technology impact statements, if they are to be useful.

The U.S. Government, through the National Advisory Committee on Aeronautics and its successor, the National Aeronautics and Space Administration, and through its military aircraft development programs, has paid for most, if not all, of the basic research and much of the applied research required by the commercial aircraft industry. The basic technological problem for the industry has been to choose among relatively known, and in some cases well proven, technologies and from these assemble a product that provides enough of an economic advantage over existing models to induce orders in a volume sufficient to justify production. In short, the risk has been primarily economic, not technological. Only in the case of the SST was the industry attempting to push out the technological frontier with a commercial aircraft to any significant degree, and even here prior military development such as the B–70 has answered many of the basic technological questions [6].

According to a recent study by Sydney Carroll, the U.S. Government has permitted aircraft manufacturers to earn rates of return on total investment on military aircraft projects that have been substantially in excess of the level required to allow these firms to attract capital. This has permitted these manufacturers to initiate commercial aircraft projects whose prospective rate of return, adjusted for risk, has been below that which ordinarily would be required. This has led in turn to a greater number of new commercial aircraft projects and consequently, to more technological change [7].

The two policies just mentioned affect the supply side. Other important U.S. Government policies have had a profound effect on the level, and more importantly, on the *timing* of the demand for commercial aircraft. And this 'market pull' has had its own substantial impact on the rate of technological change in the commercial aircraft industry.

The 'market pull' impact need not be always in the direction of stimulating increased investment in technological change. Leonard Lederman, in commenting on the original draft of this paper, has suggested that in cases when U.S. Government stimulation of demand results in demand exceeding supply, innovation can carry with it the risk of disruption of the orderly flow of output and an attendant loss of current sales. One instance in which the drive to maximize current output resulting from excess demand has indeed appeared to have had a serious adverse impact on the incentive of the industrial enterprise to innovate in spite of relatively high long-term payoffs has been in Soviet industry [8]. The net impact of demand stimulation on private incentives to innovate must be judged on a case-by-case basis. Among such policies are the following:

U.S. Government subsidy to the trunk airlines during their early days of operation allowed them to purchase aircraft in quantities that made production of new aircraft types feasible. This was particularly important during the immediate post-World War Two period. U.S. Government subsidy to the local service carriers at first supplied the carrier group with funds to buy used aircraft from the trunks. This demand drove up the prices of these used aircraft to above book levels generating capital gains for the trunklines providing them with an additional source of funds with which to purchase new aircraft. More

importantly for the trunklines, the knowledge that the aircraft they were buying would not decline substantially in price reduced the risk to these carriers of over-ordering new aircraft.

U.S. Government subsidy to the local service carriers during the early and mid-1960's was essential to providing the profits that established the credit-worthiness of these carriers enabling them to become major markets for short-haul new jet aircraft [9].

Civil Aeronautics Board regulation of air carrier fares tends to discourage carriers finding themselves in an inferior equipment position from attempting to overcome this disadvantage by offering lower fares. This has tended to produce 'bunching' of aircraft orders, since when any one major carrier ordered a new aircraft type, there was substantial pressure for his competitors to follow. This 'bunching' of orders is crucial to the successful launching of a new transport program. William Jordan in his study of the equipment-buying strategies of unregulated California intrastate carriers has confirmed this impact of regulation on technological change in the commercial aircraft industry [10].

The U.S. Treasury has established extremely liberal depreciation policies for transport aircraft. The most recent Treasury guidelines allow such investments to be written off over a five-year period for tax purposes [11]. To the extent that air carriers actually keep aircraft for a period longer than five years—and in practice most do—this policy represents an interest free loan from the Treasury to the airlines which is translated at least in part into an increased demand for aircraft.

The Export–Import Bank participates substantially in the financing of virtually all aircraft sold overseas. This participation is not limited to aircraft sold to underdeveloped countries. Airlines in Japan, Switzerland, Scandinavia, and The Netherlands have all received Eximbank assistance in financing recent wide-body jet acquisitions [12]. The availability of such financing can have a substantial impact on the demand for aircraft. The Aerospace Industries Association in a recent publication noted: "* * * the lower rate of interest made possible by Eximbank participation, plus the extended period of repayment allowed by the bank, can be quite significant to a buyer who must plan on covering all repayment costs with operating earnings. An annual cash flow reduction of as much as 25 percent below that would be required with straight conventional financing is possible." [13].

This is indeed a formidable array of programs serving to correct any tendency that might exist for the commercial aircraft industry to underinvest in technological change due to its inability to capture fully the benefits this technological change creates for society. Certain programs or policies may have the opposite effect: (i) U.S. Government regulation and subsidation of local service carriers has tended to discourage the development of aircraft designed to provide efficient service on short-haul, low-density routes. (See Eads [9] esp. pp. 125–142.) Exemption from economic regulation of a class of small operators has tended to have the opposite effect. (ii) In 1928 the Manufacturers Aircraft Association, Inc. (MAA), a group consisting of most major aircraft manufacturers, entered into a patent cross licensing agreement at the urging of the government. In 1972 the U.S.

Justice Department filed an antitrust suit against the MAA charging that the agreement amounted to a device to eliminate competition in research and development and had retarded innovation in the aircraft industry. (U.S. Justice Department press release, 29 March 1972). In spite of this, a Presidential Commission recently concluded that the United States can no longer rely on the free market to produce new transport aircraft in the quantity and variety required to meet the challenge of foreign competition. The Commission proposed the creation of an agency within the Department of Transportation that would decide when new aicraft types were 'required' and also the company that would build these new aircraft [14]. At about the same time the then-chairman of the Civil Aeronautics Board, Secor Browne, was touring the country raising the spectre that without substantial aid from the U.S. Government, no more commercial aircraft would be produced in this country after the production of the current generation of aircraft is completed [15].

These two examples show how a theory that has substantial potential use as a guide to decision-making may be used to justify bad policy if no way exists to show that the policy being touted is, in fact, inappropriate. The theory of externalities in its simplest form predicts that under a certain set of assumptions there will be a general tendency for private industry to underinvest in technological change and states that governmental intervention aimed at correcting this tendency may be proper. The practical outcome is that someone—perhaps even a party having a substantial private financial interest in the outcome—perceives that an industry is achieving a rate of technological change below the level that the particular party believes is desirable. After suitable publicity has increased public awareness that a problem exists, a prestigious panel is thereupon convened. After an appropriate interval it produces a report stating that while, of course, everyone knows that the economy would operate best if the market were left free to operate, in the particular case at hand the market has 'failed' and cannot be trusted to bring about the socially desirable result. It is at this point that the theory of externalities is invoked. In the case at hand, the fact that no U.S. commercial aircraft manufacturer is producing aircraft equivalent to the A–300, the Mercure, the VFW–614, the Yak 40, or the Concorde is taken as proof positive of market failure. No attempt is made to have a truly disinterested outside party determine whether the alleged market failure has indeed occurred and, if it has, to pinpoint the source of that failure so that a remedy designed to correct the problem with a minimum of interference in the operation of the market can be fashioned. Again, referring to the case at hand, it apparently never occurred to the Presidential Advisory Commission or to Mr. Browne that the failure of the U.S. commercial aircraft industry to produce analogs to the aircraft mentioned is perhaps a signal that the market is indeed working properly. In this industry—as in most others—supply does not create its own demand. I have been unable to locate a single case where a promising commercial aircraft was not produced due to the failure of the market properly to rate its commercial prospects, but I have uncovered many cases in which aircraft were produced after having been given massive direct governmental aid on the grounds of market imperfections and in which the pessimistic judgment of the market

has been fully vindicated. Indeed, the entire postwar British experience can be viewed in this light.

Thus far attention has been confined to problems raised by the fact that conditions in industry do not generally correspond to those that must exist if the simple theory of externalities is to provide an unambigous guide to the appropriate level of U.S. Government intervention in the process of technological change. We turn now to the dangers that may exist if a premature attempt is made to put U.S. Government science and technology policy on what might appear at first to be an economically more rational basis.

There has been much concern expressed by the scientific community that the fiscal 1974 budget implied a downgrading of basic science in favor of applied reesarch. Science policy was said to have come "under the reign of economic decision-makers." ("It's Austerity Time for Basic Science" [16]. The subhead of this article states: "The academicians are out. And so is the emphasis on backing basic research".) William O. Baker, president of Bell Labs and head of the recently created Science and Engineering Council, stated: "The Administration is anxious to apply to government more of the values of the business community—efficiency, cost-effectiveness, chains of authority." [17]. The thrust of the fiscal 1974 budget was said to have been toward "maximizing short-term payoff in research" in energy, health, and transportation—areas deemed to be of "special national concern." [18].

It is difficult to short out the rhetoric from the reality to learn just how significant any such policy 'shift' was. (For a view that the concern over the "shift" is overstated, see Wade [19].) If history is any guide, at least some of what was publicly called a 'shift' merely represented the repackaging of existing programs so that they appeared to be more in line with current domestic political concerns. Yet the highlighting, even if only with rhetoric, of the desire to make U.S. Government science and technology policy more 'businesslike' raises a valid concern over what is likely to be the result.

One prediction that comes out of the simple version of the theory of externalities is that the degree of underinvestment by private industry—and hence the size of the proper role for government—is greater the closer one gets to the basic research end of the technological change spectrum. As the President's Council of Economic Advisors notes in the passage quoted earlier, this is because the returns to basic research are unlikely to be fully perceived at the time the research is being carried out, are even less likely to be fully capturable by the funding party, and are likely to be realized only in the distant future. The further toward applied research and product development one proceeds, the more likely it will be that the person funding the rsearch will be able to perceive and capture fully the total social benefits resulting from his outlay, and hence, the less likely it will be that there will be an underinvestment in technological change.

All of us, and most particularly we economists, favor allocating our scarce resources where they will bring the greatest return to society. And it certainly is possible that in our desire to see that science not go undernourished we have instead overfed it so that some slimming down of unpromising programs is justified. But how should an evaluation as to whether such a 'slimming down' is needed be conducted? *If* economists currently were able to measure externalities, this could be

done relatively easily. The net social rate of return (the social rate of return minus the private rate of return) to funding additional research in the basic mathematics, physics, biology, or astronomy could be compared to the net social rate of return from additional government aid for the development of 'people movers', quiet jet engines, or breeder reactors. Scarce governmental funds could then be distributed so as to maximize their contribution to society's well-being.

The process just outlined reveals the problem. As of today, economists *cannot* routinely measure externalities. The necessary tools have not been developed. Therefore, to allocate U.S. Government funds on the basis of demonstrated potential economic payoff is bound to lead to an underfunding of basic research by the government for one of the same reasons that business itself underfunds basic research—because its economic benefits cannot be fully perceived.

This unfortunate tendency is likely to be accentuated by the fact that the payoff to the government from support for applied research may be overestimated. For reasons that have already been mentioned, supporters of funding for basic research are often unable to point to any specific economic benefits that will flow from their projects. Supporters of applied research, however, usually can point not only to tangible goods or services which will exist if their projects are funded and are successful. They can also point to 'secondary' benefits that the production of these goods or services will create—employment and exports, for example. It does little good to point out that in a majority of cases, these 'secondary' benefits can be shown, if subjected to disinterested critical analysis, to be at least overstated and sometimes to be entirely spurious. Their claimed existence, and particularly the fact that they can be stated in dollar magnitudes, lends a powerful additional appeal to the projects they are associated with. Furthermore, even the direct benefits of governmental support of applied research lend themselves to overstatement. In many cases government aid merely acts to speed up the availability of some new technology. However, supporters of government funding for such projects invariably credit government support with bringing the technology into existence rather than properly counting only those additional benefits that would accrue to society from having this technology available sooner rather than later.

Therefore, it is possible that if government support for science and technology truly were put on a more 'businesslike' basis before the tools exist that will allow the full social benefits of research to be measured, basic research might be underfunded to a *larger* degree than it would be if its support were left entirely to private industry. This is not to suggest that this outcome will be likely. Despite rhetoric about shifting U.S. Government R. & D. funding from a 'supply-push' to a 'demand-pull' orientation, it is to be expected that most of the actual decisions concerning basic research support will continue to be made (and appropriately so, given the current state of knowledge) on the basis of rough rule-of-thumb calculations about what proportion of the nation's resources it can 'afford' to devote to basic research.

However, the distortion that a premature 'rationalization' of U.S. Government support for science and technology might introduce

would not be limited to basic research. Applied research projects differ in the real (or claimed) externalities they may generate. It is predictable that once the simple form of the theory of externalities is comunicated to and understood by the science and technology community, it will be increasingly misused as interested advocates vie with each other to uncover 'benefits' now being lost to the nation because the program they are supporting does not now exist. The extreme difficulty of measuring externalities, exemplified by the problem of measuring the benefis of basic research alluded to earlier, will contribute to this tendency. If care is not taken, 'externalities' will be discovered primarily where it is politically advantageous to discover them. I do not mean to fault the science and technology community for doing this. They will be following a well trodden pathway. Water resource development and urban renewal are but two examples of programs where U.S. Government funding was justified largely on the basis of "externalities' but where the externalities have, in retrospect, proved to be largely illusory.

As noted earlier, a good deal of the responsibility for this state of affairs, and much responsibility for seeing that it does not happen, rests with the economics profession. Too often, as in the case here, economists have discovered an economic principle, have publicized it and achieved its acceptance, and then have forgotten about seeing to its proper implementation [20]. This can be attributed in part to the current reward structure in the profession. The most prestige is accorded to developing new theory. Less prestige is attached to a skillful job of publicizing a theory and securing its acceptance. Little or no prestige attaches to performing the difficult work of making the theory operational. Yet if economics is to continue to be useful as a policy science, some way must be found to assure the quality of the product once it has passed out of the realm of the economics journals and into daily practice. Economists must take a greater interest in translating abstract economic concepts like externalities into language that policy makers can understand. They must explain clearly how externalities can be recognized, and, perhaps more importantly, what externalities are *not* as well as what they are. They must carefully communicate their assumptions and provide the tools that will allow the policy makers to discern how well these assumptions are realized and in what directions the policy implications of the simplified theory are altered if these assumptions are not fully met. Second, some method needs to be found, such as was done in the case of the U.S. SST project to 'blow the whistle' when the misuse of basic economic concepts becomes gross. Perhaps Dr. Alan Ferguson's new Public Interest Economics Center is a prototype of such an institution [21]. But to retain credibility such an institution must be careful to avoid the lure of partisanship. Whistles must be blown when economic concepts are misused to justify programs that are highly popular to the person blowing the whistle as well as when such concepts are being used to bolster programs the 'whistle blower' personally finds distasteful. This is the essence of professionalism.

A final observation. When considering the replacement of the judgment of markets with the judgment of men—such as has been pro-

posed in the commercial aircraft industry—a full understanding of the past history of such attempts must be kept in mind. Markets are undoubtedly imperfect, but bureaucracies also suffer from major imperfections. The relevant comparison, therefore, is between the results that will likely be produced by two highly imperfect systems.

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"Show Me A Scientist Who's Helped Poor Folks and I'll Kiss Her Hand"

By Thomas J. Cottle

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Most of the apartment houses on Taylor Street behind the South End of Boston were once fashionable homes owned by wealthy fami-

lies. Years ago when these well-to-do white families moved out and poor Black families, many of whom had migrated from the South, moved in, the houses were converted into apartments, sometimes with as many as four dwelling units on each floor. Turrets and towers that remain on the roofs and unusual designs cut into the stone facades of the buildings remind the present residents that once the wealthy lived here, once artisans built buildings here, once life was rich here.

Today Taylor Street is one of many streets in a poor neighborhood hidden from the main currents of Boston by the developing South End area. Still, the children who play against their parents' wishes on some of the rooftop towers on Taylor Street claim they can see all of Boston, particularly the taller buildings and even those across the Charles River belonging to the Massachusetts Institute of Technology. They report, for example, sighting what looks to them like weather-predicting equipment atop one of the MIT buildings. Several of these children have decided, moreover, to explore the Institute, see it up close, prowl about the buildings. The idea of scientists working so near to where they live intrigues them, and because I come from MIT to visit with them, it is only natural that they would tell me of their plans and turn our conversations to the work of scientists.

One of the boys who climbs to the top of an apartment house on Taylor Street to spy on MIT is ten-year-old Keith Downey. He is a thin young man with large round eyes and a high forehead. He never goes anywhere without one of his hats, which disturbs his parents who practically have to pull them off his head when he enters his home.

"I've got my thinking hats," he told me one afternoon. "I think science with them on, particularly my gray one. I can go up there to the tower and look at where those scientists work and just about get myself to imagine what they're doing."

"Which is what?" I asked him.

"Seems to me, they spend a lot of time working on rockets. They want to explore the planets, maybe even have people stay up there in space for a long, long time so they don't ever have to come back down for fuel and all the other stuff they need, like their supplies. They could work up there, I'll bet, for whole centuries, have their families live up there, have their babies born too, and when people die, dump the bodies into space. Up there you don't need to bury people, you know. You just throw them out of one of your spaceships. It's easier than the way we have it here. Like, when my grandfather died, my father said he didn't know whether it was cheaper to have him alive or bury him. People who buried him and did all they had to do at the chapel on Caldwell Avenue charged my father so much money, he said he couldn't pay them. He told my mother one night they might have to throw grandfather into the ocean. She got real angry 'cause it was her father that died'. They didn't know me and one of my sisters were listening to them fighting. They got the money though, from some loan company which my father says is no better than stealing since he'll be paying them back for the rest of his life. Not really, maybe, but for a long time. But I figured that if scientists really wanted to do a good thing for my family and folks like us, they should figure out a way to get rid of people when they die so that it doesn't cost us so much money to take care of all the dead corpses."

When Keith Downey walks with me on the streets of his neighborhood, he gives the appearance of being oblivious to almost everything. He jumps curbstones, peers in windows of grocery stores and restaurants hunting for his friends, but he does not seem especially sensitive to the environment. When I go with him into the downtown areas, however, and especially to college campuses, he begins to see and touch everything that he can. We will be walking together and Keith will rub his hand along a building or fence feeling the metal or stone or brick. He may pick away at chips of mortar and rub them between his fingers, or hit a pipe to listen to its special sound. When we visit MIT, his look is one of awe. In the nutrition laboratories, for example, where one of our recent visits took us, a graduate student explained to him the nature of some research on artificial food substances. Keith was deeply interested in this work.

Later, on the trolley going home he told me, "You see how much scientists do for people. That food laboratory we saw there has to be an important place. When they get done with their work there won't be a single person in this country going to starve any more. Now the President of the United States, he has all the power and all the money, but he doesn't have the brains like those folks at MIT. They're the ones who'll do the work so that pretty soon, like that one man said, a person can swallow a couple of pills and have all the food he needs that day. Or maybe that week too. That's the day, man, I want to see. Come into the kitchen and tell my mom, give me the breakfast pill, mom. She'll hold it out for me and I don't have to come home again till supper, especially if I can stick my lunch pill in my pocket too. That man at MIT, he's got the right idea. Never go hungry, and never have to waste all that time sitting around at the table listening to all your baby brothers and sisters screaming in your ear while you're trying to get something to eat. Scientists, man. There can be *nobody* on the earth doing better things than they are. What else did we see there, Tom?"

I reminded Keith of the medical laboratory we had visited where serological experiments were just getting underway along with some examinations of heart muscle. The latter required the use of dogs who later on would be sacrificed. Keith had wanted to see the insertion of a canula into a major artery. We had watched together, robed in white lab gowns, and only once did he wince. The rest of the time, almost one hour, he stood transfixed, fascinated. Hhere again a graduate student explained the research to him, but now, alighting from the trolley and walking the nine blocks to his home, his fantasies began to play on his recently acquired knowledge.

"Here's what scientists are going to be able to do, maybe even have it done before I die. They'll have a brain or a heart that's dying. The person isn't dying, see, just his brain or his heart is. Or maybe his stomach will be rotting away. I heard of a lady whose stomach did that," he interrupted himself. "The doctors said it was because she never ate the right foods. So they cut her stomach out and then she didn't have one. But see, when all those scientists we've been visiting get done, they'll just look you over, you know, X-ray you and whatever they do, take your heart out and put a new one in."

"They sort of do that now, don't they, Keith?"

"Yeah, but those folks are dying. Scientists and doctors have to work a little more, just a little bit more, then they'll really be able to keep people from dying. If one part wears out, they'll put another part in. It's just like fixing a car. You got a bad nose, they'll take it off and put somebody else's nose on you." This idea struck him as particularly amusing and for almost a half a block he was convulsed with laughter.

"You know what I wish scientists would do," he said when at last he composed himself, "I wish they'd build cars that could go as fast as rockets, only not go up into the air. I get kind of scared thinking about going into those rocketships, although maybe I'll be an astronaut. But I wish they'd make cars go faster and fix it so no one would ever crack up. Then I wish they'd make pills to fix you up when you're sick." He was letting his mind wander over ideas he had played with during our visit to the Institute, almost hoping he would hit on some scientific invention that would wholly please him.

"I got it," he shouted out finally as we crossed over to St. Mark's Street. "Dentists! Scientists are going to have to fix your teeth with pills and drugs that don't hurt. Maybe they could put you to sleep before you even leave your house going to see them. Get rid of that pain, man, and you've done something for everyone. Everyone that goes to the dentist, that is."

"The lives of scientists really intrigue you, don't they, Keith?"

"Better believe it, man." He was aglow with excitement reflecting on our excursion, as well as on the view he gets from his special tower on Taylor Street. "I sure would like to be one someday. Working in those laboratories like we saw, inventing things that no one has ever heard of, maybe get famous, discover something that would help people. Get rid of dentists," he smiled, "help babies get born right, make people strong."

ANSWERS . . . SOMEDAY

"We didn't even talk yet about energy," he went on. "My science teacher tells us we got this, what they call an energy crisis. If we run out of oil and gas, he says, we aren't going to have any cars or heat in the houses, you know. The way I see it—I mean, not me exactly, but we talk about it at school—scientists, maybe me and my friends someday, are just going to have to find the answers, otherwise a whole lot of people are going to die. If you don't have the right food you die. If you don't get warm enough you die too. And if you can't drink the water 'cause it's all polluted with stuff, you're going to die too. So scientists will have to send more astronauts into space just to see what they can find up there, or send those other folks under the ocean in submarines to dig for oil. Mr. Cleary, he's my science teacher, he says the secret of the earth lies in the oceans and the seas. That sounds right to me. I'll bet scientists could find a way to get some of the heat from the sun and get everyone warm every day of their life. I'll bet you could take all the heat of the sun away from it every day and still have enough sun the next day. It would just heat itself up all over again during the night. Maybe they'll find something in those rocks they brought back from the moon too. Maybe they can get something out of them that will cure us of something or other." He was scratching his head.

"Scientists, you know, have to keep hunting. It's slow work, just like we saw over there today. You have to be patient, which is why I'm going to be a scientist. I've got the patience for it. You take a problem and you stay with it. Day after day, year after year, if you have to. But you don't quit until you find the answer you're looking for—until you can find a way to cure somebody of something, even one person. Everybody's different, you know, so you need different scientists working for different folks. You understand?"

I nodded my head yes.

"We got to go back there someday real soon, Tom."

"We will."

"Can we?"

"Of course."

" 'Cause there's something else we have to know about."

"Which is?"

"I don't know what you call it, but they do these experiments on babies and children, real little kids, you know, just born, to see how they're going to be when they're adults. What they got in them from their parents, I mean."

"Genetics," I said.

"Yeah. Genetics." He repeated the word with reverence in his voice. " 'Cause you know some people got things wrong with them when they're born and only scientists can figure why they got them, and what we got to do to get rid of those wrong things."

"And lots of people have lots of not-wrong things with them," I said clumsily, "which geneticists also study."

"Geneticists. I think I'm going to be a geneticist," Keith said as we reached the corner of Taylor Street. He was looking up at the rooftops where he and his friends play. Children his age were running about on the street, taking advantage of an unusually warm March day, but Keith took little notice of them. "I can see your college from right up there," he said, pointing to his favorite tower. Suddenly I saw the image of Galileo working alone in his private laboratory.

"That's the tower, eh, Keith?"

"Right up there, man. That's where my career in science is going to start, with your help."

"With *my* help?"

"You got to keep taking me to those places. I've got to get prepared. You got to help me get books and pass all my courses, which are really going to get hard in a few years, like in high school. I'm counting on you to stay there at MIT and get me in there with you so I can become a scientist. Not a pretend one like I am now, but a real one. And if I get sick, you got to get me a doctor and get me the money, and get me all I need."

His friends calling to him to join them finally made it impossible for us to finish our talk. Within a few seconds he was running off, earnestly making me promise to take him to another college in a week or two, and reminding me of all the things I was going to have to do for him in the next ten or fifteen years: "I'll take it from there," he shouted as he disappeared into the burger shop on the corner of Taylor and South Plaine. "You got to take care of me, man, keep my head in one piece," was the last thing he said to me that afternoon.

Several minutes later I entered the home of Arthur and Estelle Downey, Keith's parents. As their apartment is on the top floor of one of the row houses, the stairs leading to the roof and towers are located in the hall just outside their front door. The apartment is unbearably hot in the summer and cold in the winter since the insulation in the walls and ceiling is inadequate and the basement furnace five floors below lacks the power to keep the few radiators warm. And since the heat is turned off at ten at night and not turned on again until seven the next morning, the hours of even minimal comfort are precisely those hours when no one is home.

"So you took my boy to see all the famous scientists this afternoon," Estelle Downey said, greeting me at the front door. As usual I had asked her permission to take Keith on the excursion, and she made certain to be home on our return. "Seems like all he's interested in these days is science and scientists. Books and television, and now he's got you, and MIT. Sure was a perfect day for it, wasn't it?"

"It was that," I answered my friend of five years and the mother of six children. I sad down in the living room. Estelle tidied up as she spoke to me.

"That boy and his love for science. Do you know how hard it is for me to keep my hatred of those folks away from his hearing?" She shook her head as she beat the dust out of the couch pillows. "Scientists," she muttered. "Rich folks is what they are, no different than all the rest. Sitting over there where Keith spies on them, playing with this and playing with that. Making up problems where problems don't really exist. Making things complicated when really what we need done is so simple a child could understand." Her tone was bitter. "Every day I read in the papers about the money they get to do all those expirements with whatever you call what they do over there. And not only over there at your place, but all across the land. In all the colleges! What I want to know is what good are they doing for this country? What good are they doing for Black folks? and poor folks? What are they doing for folks who haven't learned to speak English yet? They just go on playing with this and that. What do they care that we have children dying over here in this part of town, and that boy of mine is sitting up there in his tower, ready to fall on his head onto the street down there, looking over across the river as if it were the promised land?"

"Believe me, Tom, Arthur and I are very careful not to say a word in front of him, but don't you honestly think those folks over there know as much as anyone needs to know about things now? I mean they got hospitals and doctors in this city, and they got colleges and all kinds of training schols. They even got a museum for science. Isn't that enough? What are they doing with all that money?" She was standing opposite me clutching a pillow to her breast. "What do they find to do with all that money? Making their experiments and all, and just who do they experiment on? Is that a question those scientists folks would like to answer?" She walked past me to the kitchen where she found a copy of the morning *Globe* on a chair. "On the second page, right here. You see that?" She folded the paper over and thrust it out to me. "Texas scientists this time, experimenting

on babies. Newborn babies. Three hundred Black babies, and making it out they were like little animals." She smacked the newspaper as she walked away from my chair.

"I read it this morning, Estelle."

"Well I hope it made you sick. They can't seem to find Black children for the schools, they can't find them when they want to do something nice for them, but when they do their experiments they find them all right. Three hundred of them! Now, you going to tell me that's fair?"

"Of course I can't," I said.

"You're damn right you can't. Not a scientist in this country right now seems interested in doing anything for Black folks. You show me a scientist who's helped poor folks and I'll kiss her hand." She turned quickly to look at me. "You get what I mean?"

I nodded. "You want me to stop taking him to MIT?"

"Of course I don't. What would that solve? We're happy with any little break we can get. But I *would* like you to tell me why it is we can't get medicine and doctors and dentists and the right food? I *would* like you to tell me how come they got buildings over there, Keith says, for their experimenting and we don't have the money to keep on in this building we live in. That's what I'd like someone like you to tell me. I'd like someone to tell me where they get the money to train all these scientists and have to cut out the funds on every single project that just might help poor folks in this community." She paced back and forth in the small room, and each time she passed the window she peered down onto the street.

"My God, if what Keith says is even halfway the truth—and that boy doesn't lie to me or to his father—then they got better conditions for the dogs they do their experiments on than we've got for our *children*! You know, they actually feed them better, clean them better, take care of them better, and you want to know something, if those dogs and mice get sick, they cure them. It's not like here. What have we got here, scientists to help us? The hell we do. Child gets sick here, he's worse off than one of those dogs. Now, you're supposed to be some kind of a doctor or philosopher and I'm just an uneducated woman coming up here from a tiny farm in Georgia when I was eight years old. So we ought to be able to agree that I don't know and you do. So suppose you tell me how this society can keep going when your dogs on that side of the river are in safer hands than our children on this side of the river? You go ahead and answer that question for me. That's the way you got to look at science, you know," she pointed at me, still pacing across the far end of the small living room. "It fits into the society somehow. Science can't be something so special that it doesn't affect the rest of us. Dogs and children are part of the society, but the way I look at it, the children right here on Taylor Street aren't getting as good a treatment as the animals." A last she turned to me and stood still.

POLICEMEN FOR US, SCIENTISTS FOR YOU

"I blame the scientists," she said, seeing that I wasn't about to speak. "I blame them all. They're specially educated, every one of them. Every single one of them has degrees, like a doctor or whatever.

Just like you have." I said nothing. "They know what's happening in America. They know the children here are dying from the lead they eat in that paint. Not just Black children either. And they know children are dying because no one takes their tonsils out in time or their appendix out. Scientists, doctors let that happen. Scientists don't make cheap medicine for us. They don't help us get the vitamins we need, or the medicine I need to give the boy of mine you like so much so that he won't have one of his epileptic seizures at any minute. How many times have we gone without it? How many times have *you* carried him up those stairs with his head wagging back and forth and his eyeballs rolled back and his tongue halfway down his throat? You tell me how many rich children have to go through that? You tell me how many children of scientists go hungry every night, or stay in this hot city every month in the summer. How many rich folks can't get the operations that we need all the time and can't get? Scientists get the money to make their experiments and design all that research of theirs, but we don't get to see a dime of it. We just wait in those emergency rooms begging some twenty-one-year-old policeman to cure our children. Maybe that's what it's all about. You all got your wise old scientists, and we all got our policemen, most of them not much older than my oldest boy. Policemen and doctors, cause we don't have anybody else. While your dogs do better. I sure as hell can tell without seeing them that they eat better." She was pacing back and forth again, stopping momentarily to glance out the window at the children and the traffic five stories below, then looking about the room trying to decide if she wanted to continue her cleaning.

"Where's my Keith now? Eating something? Eating grease and junk like that? Are they going to find some illness in him tomorrow, or five years from now? They going to guarantee me that scientists are going to help him? They going to guarantee me that even if they know *how* to help him, they can get their research over to this part of the city? Maybe he'll cut himself and bleed, out there somewhere no one can find him? Scientists! How they going to help that? With bombs? With rockets to the moon?" She spun around and looked straight at me. "I want them people down here, not walking around on the moon. I want them in their rich folk's laboratories working on sickle cells. That's right," she shook her head, "sickle cells. Let them work on *our* problems for the next ten years. Where's the money for that? Where's the money for curing *our* illnesses? Where's the research for Black folks, instead of for white folks' dogs and white folks' mice, and all those little pigs they got too?"

"And something else, which your face reminds me of too:" I sat up straight in the chair. "What about your kind of scientists, Tom? Don't you belong to the certain kind of scientist too? Telling folks in the newspapers how we live and how our children are doing in school. Studying all these bussing programs and deciding how well they're working or not working. Aren't you part of that?" Her voice had grown quiet. "Something tells me you are. Something tells me that *we* may get the sickle cells in here," she poked at the veins in her forearm, "but *you* folks are responsible for making the whole community filled with bad cells, evil cells. You scientist folks, it's up to you to

cure the society. You're the ones who made it sick, and so far you all are just looking at it, doing your research and going about your business, not caring a damm about whether my child lives or any other child lives if you can use him in some experiment.

SCIENCE AND SLAVERY, 1974

"Let me tell you, there's all kinds of experiments with infants, with little children, with children as old as Keith, that are going on in all these colleges. You can read about them in the paper. And in every one we're always being experimented on. We're always the dogs. You mess around on us, and then you leave us to die. But not a word comes from you, not a *I'm sorry*." She walked toward me and pushes her finger at the newspaper I still held in my lap. "Three hundred Black folks gave their babies to those scientists. That means all over the country they're experimenting. We never get to say a word about it. Or if we ask, we can't be sure they're telling us everything that's going on. We're just your dogs waiting for you to ply your rich games. But you never show your white faces around here. You never even say *I'm sorry, I'm sorry for what's happening. I'm sorry that we got our white folks walking on the moon while you Black folks are falling on your beds sick with hunger, and your stomachs rotting. I'm sorry that your boy is an epileptic!*" We were staring at one another now. But with all the anger and feelings of betrayal, she still could sense what it was I naively hunted for.

"You don't have to worry, Tom, I'm not crying about this anymore. We've been your slaves so long, it's like nothing's changed. Only now," she said glancing at the paper, "they call it science. Everything in the name of science. But anyway you cut it, you're the masters, we're the dogs, and I just got to wait and see whether a seizure someday will take my boy away from me. And I suppose," she finished quietly, "away from you too."

"New Spurs for R. & D."

By Murray L. Weidenbaum

From *The Economy*, March 1974, p. 11.

As America turns to development of new domestic energy sources, we must realize one thing: that recent changes in government priorities have had a strikingly negative impact on the research and development we need. That impact is unintentional, but very real. As long as the programs with a large R. & D. content (notably defense and space) were also the priority areas, R. & D. had a preferred position in the federal budget. But the shift in recent years to programs with virtually no R. & D. content, especially Social Security and Medicare, has changed all that. Because of its key contribution to domestic well-being as well as to our international competitiveness, R. & D. needs to be given priority in its own right. Otherwise the United States will continue unwittingly to downgrade the importance of perhaps the most effective force for national progress.

It is time to look closely at the various mechanisms that can be used to promote research, development and innovation in the private sec-

tor—for energy as well as other key areas. These mechanisms include direct support of R. & D., Federal procurement, grants to States and localities and tax and credit subsidies.

Clearly, we need new types of Government spending programs, more oriented to R. & D. For example, some Western European governments share with private companies the cost of developing, producing and marketing new products and processes. Others offer "launching aid," for approved projects. That is, they share the losses that may arise during the first year or two after a new product is introduced. These government subsidies can be recovered later. Thus, in the case of successful exploration or product development, the Government may require royalty payments equal to some percentages of sales or profits.

In the other industrialized nations, tax benefits are the most frequently used method of encouraging private research, development and innovation. Although the U.S. tax system hardly rivals them in the scope of such incentives, it is not devoid of them either. Our largest incentives are at the manufacturing stage—liberalized depreciation and tax credits for investment in new plant and equipment.

Only a few tax provisions are expressly designed to encourage research and development—such as the deductibility of equipment purchased for R. & D. work. Additional tax incentives to promote technological innovation have been suggested from time to time. The most frequently encountered idea is a partial tax credit for R. & D., similar to the existing investment credit.

Other Government financing devices also could be developed. Although few existing Federal loan programs have a significant R. & D. content, Federal credit lends itself to encouraging R. & D. and product innovation. As in the case of existing small business programs, Washington could work through intermediaries. TESBICs (Technical Entrepreneurial Small Business Investment Corporations) could be set up like the recently organized MESBICs (Minority Enterprise Small Business Investment Corporations). MESBICs are designed to encourage holders of private investment funds to make venture capital available to minority enterprises, with the Government sharing some of the risk.

In view of the continued need to hold down Government spending, attention should also be given to mechanisms which do not affect the budget directly. Loan guarantees, for example, do not require any substantial amount of expenditure, or loss of revenue. The administrative costs are typically small and more than amply covered by user fees.

Over the years, the Federal Government has embarked on several business-type enterprises that subsequently have been converted to private ownership, such as Fanny Mae. COMSAT is the rare case in which Washington took the lead in setting up a privately owned corporation, particularly one designed to use the fruits of Government-supported technology. Along this line, proposals have been made lately for setting up a COMSAT to promote development of new domestic energy sources.

On a more modest scale, the Federal Government might consider following another Western European example: fostering cooperative technical associations in a given industry. To a limited extent, such

associations now exist in the United States. However, antitrust and tax considerations generally require that the results of their R. & D. be put into the public domain and thus become available to those who did not pay for the work. Some changes in legislation might result in greater use of this cooperative device, particularly if there was some Federal sponsorship.

One way to encourage R. & D. would be to provide that the resulting patents be made available free to all the industrial participants. To assure that the benefits are widely transmitted, other companies could be allowed to use them, but only after payment of royalties.

Whatever approach we take, the Federal Government should not take on the bulk of the responsibility for determining the areas of R. & D. to be emphasized. Nor should it decide on specific projects and performers. It would be better to use the tax alternative because it relies primarily on market forces and normal business incentives to allocate resources. In this way, R. & D. does not become free, but merely cheaper than it would be in the absence of Government aid. There is still pressure on the firm to make sure that it is getting a favorable return on its R. & D. investment.

But no single mechanism should be relied on exclusively in designing a program to enhance technological innovation—be it tax or other incentives. What is needed is experimentation with each of several methods. As in any new undertaking, there is no substitute for trial and error—and a modicum of good luck.

"Science and National Policy"

By Patrick E. Haggerty

From *Science*, June 28, 1974, volume 184, p. 1348.

All of us, of course, are well aware that there is a strong undercurrent in our industrial societies of antiscience, antitechnology, and antiindustry, and antieconomic growth attitudes. Yet, when one examines the needs of the overwhelming majority of the citizens of this world, it is difficult to conclude otherwise than that more and better science, technology, industry, and econmic growth are required. I concede that we have not always been sufficiently conscious of the overall quality of life, but I would argue that only through a vastly improved knowledge of ourselves, our environment, and our universe are we likely to be able to attain and sustain an improved quality of life. I would further argue that economic growth is anything but obsolete and that such almost universally accepted indices of quality of life expectancy, infant mortality, literacy, and years of schooling completed all correlate strikingly with even such an admittedly limited measure of economic welfare as gross national product per capita.

MEASURES OF ECONOMIC WELFARE

Writing in 1972. economists William Nordhaus and James Tobin attempted to answer the charge by critics of economic growth that we have not been growing at all in any meaningful sense. Because, along with all economists, they are aware that gross national product is not a very good measure of economic welfare, they constructed a primitive and experimental measure (MEW) in which they attempted to allow for the more obvious discrepancies between gross national product and economic welfare. Among other things, they imputed dollar values for the services of consumer capital, for leisure, for the product of household work, and subtracted some of the disamenities of urbanization. They concluded that in the United States mean economic welfare grew at 1.1 percent per capita per year over the 30 years from 1935 to 1965 as compared to 1.7 percent for net national product; that while MEW has thus been growing more slowly than net national product, it has been growing; and that "the progress indicated by the conventional national accounts is not just a myth that evaporates when a welfare-oriented measure is substituted" [1].

Unquestionably, burgeoning populations make it more difficult to improve economic welfare and quality of life. Yet, it is anything but clear that the situation is hopeless. A net reproduction rate of 1 will produce zero population growth when a suitable population-age distribution is attained. The net reproduction rate in the United States dropped from 1.75 percent in 1960 to 1.2 percent in 1967 and an estimated 0.96 percent in 1972 [2]. Even with a net reproduction rate of 1 or below, the population of the United States would go on growing slowly for another 25 or 50 years while the bulge in age distributions, which is a product of our more fertile years, dissipated. Over that span of time, the population of the United States would level off somewhere around 250 million, hardly a catastrophic number.

Nor are we alone in this trend toward net reproduction rates at 1 or below. Intrinsic annual population growth rates have been dropping steadily among most of the industrialized nations, and it is highly probable that the average for the entire industrialized world for the year 1973 will show a net reproduction rate of about 1. It is true that the intrinsic annual population growth rates of the underdeveloped countries are still relatively high, but their population growth rates indicate a steadily downward trend, and there is no reason to assume that they will not continue to do so. It is also important that, since 1965, the average growth in real per capita gross national product in the less developed countries has been about 3 percent and, in 1972 and 1973, was only slightly behind that of the developed countries.

I would conclude that while the increasing population pressures on the resources of the world do present real difficulties, they are not such as to be unsolvable, and that more and better science, technology, and growth in economic welfare are vital components in meeting these difficulties.

ECONOMIC GROWTH JUSTIFIED

Nor is it likely, as postulated by Forester in *World Dynamics* [3], or as indicated by the Club of Rome's study [1], exhaustion of the world's resources in the very near future will apply catastrophic limits to growth. All these models ignore the functioning of the price system,

which is the main mechanism in our economy that forces the gradual transfer from resource-intensive goods to other things, and hence automatically works to reduce requirements per unit of national output and does so steadily and gradually with time. It is true that there are defects in the market system in that the cost of such disamenities as air pollution, water pollution, noise, and visual pollution are usually not encompassed. But surely this is not an irreparable defect, and our national attention can be much more profitably directed to curing these deficiencies than to the incredibly expensive and self-defeating demand for zero economic growth.

Indeed, for most of the people of the world, the choice is very clear. They will organize their societies to achieve what they feel we in the United States, Europe, and Japan already have. Further, since here in the United States, as recently as 1971, about half the families in the country had incomes of less than $10,000 per year, and almost one-fifth had incomes of less than $5,000 per year [4], the overwhelming majority of our own citizens will not see themselves as having attained an affluence sufficient to accept, other than by force, mechanisms of political and social organization that would limit the future growth of our economy sufficiently to prevent their attaining an appreciably higher level of material welfare. Thus, I would conclude that the need and the pressures for growth are not only great, but completely justified.

Edward Denison has made extensive studies of the sources of economic growth in the United States and in other countries. He found that, over the period 1950 to 1962, 58 percent of the United States' increase in national income per person employed came about as a consequence of improved knowledge, combining the impact of education on our labor force and such applications of knowledge as technological innovation and improved management [5]. Denison's findings are based on the usual national income accounts with their admitted deficiencies in measuring economic welfare. Yet, it will almost certainly be true that knowledge will play an even more significant role in attaining a satisfactory growth rate in true economic welfare, which by its very nature must be much more complex than attaining satisfactory growth rates in the gross national product.

Until early 1973 there was in the Executive Office of the President of the United States a science advisory function that included the President's Science Adviser, the President's Science Advisory Committee, and the Office of Science and Technology. In January 1973 the Office of Science and Technology and the President's Science Advisory Committee were eliminated, and the remaining functions were transferred to the National Science Foundation. Dr. Guy Stever, director of the National Science Foundation and a fellow guest tonight, was made the President's Science Adviser.

I am sure that many of the science and technology activities of significance to the Executive Office of the President can be handled as well or even better with this new mechanism, but I am also convinced that science and technology are such significant elements in our overall culture and so vital to economic welfare that the complete elimination of the science advisory mechanism from the Executive Office of the President would appear to have been unwise.

I certainly would not suggest restoration of the old science advisory mechanism. Instead, I would advocate an approach which would involve science and technology and the forces of knowledge generation and diffusion more intimately in the policy-making activities of the Executive Office of the President.

After extensive debate on the economy following World War II, Congress enacted the Employment Act of 1946, which created the Annual Economic Report of the President, the Council of Economic Advisers, and the Joint Economic Committee of the Congress.

NATIONAL DEVELOPMENT ACT (OF 1976)

I suggest that there would be no more fitting way to celebrate the 200th birthday of this nation than with a new act, the "National Development Act of 1976," as a natural evolution from that Employment Act of 1946.

As I envision it, this new National Development Act would declare that it is the continuing policy and responsibility of the federal government:

(1) To seek for every citizen an ever improving standard of living defined in the full context of quality of life as well as material affluence.

(2) To encourage all practicable means to foster and promote free, competitive enterprise to fulfill needs of our citizens, for goods and services.

(3) To use government intervention, but with caution and understanding, to modify the market economy, to affect the price structure of goods and services so they reflect the value of such public goods as the environment, or to impose overall regulation where the welfare of society (such as for health or safety) is concerned.

(4) To require modes of federal intervention that will avoid government ownership of facilities and minimize direct government employment of workers.

(5) To use all practicable means consistent with its needs and obligations to assure that there will be useful employment opportunities including self-employment, for those able, willing, and seeking to work and to promote maximum employment, production, and purchasing power.

(6) To conduct its affairs and interventions so as to provide a stable and growing economy with a minimum inclination to inflation.

(7) To encourage a broad enlargement of educational opportunities with emphasis on equal opportunities for all men and women throughout life, including especially combined work-learning programs aimed at consistently upgrading the skills of workers everywhere and to their broad cultural betterment.

(8) To foster the growth of knowledge throughout the society in all fields including science and technology, art, and the humanities with particular emphasis on those basic areas vital to the continued economic growth and social development of the United States, including the use of research and development as key tools in attaining national objectives.

The National Development Act also would call for the President's submitting to the Congress in January of each year a national development report, reviewing the overall quality of life in this country,

including not only the overall economic performance in such usual parameters as gross national product and net national product but also a broad variety of such necessary aspects of quality of life as health, an improved environment and educational and cultural attainment. It would establish a council of national development advisers in the Executive Office of the President comprised of five members, each exceptionally qualified to analyze and interpret developments in economics, education, science, and technology and to appraise programs and activities of the government in light of the policy declared by this act. The act would also create a joint development committee made up of eight members of the Senate and eight members of the House of Representatives to make a continuing study of matters relating to the national development report and to make recommendations and findings to the several committees of the Congress dealing with legislation requisite to advance the policies established by the act.

I believe that the public debate which would accompany the preparation for this National Development Act of 1976 and its enactment would serve (i) to inform the citizens of this country as to the base of their material affluence and quality of life, (ii) to point to approaches which are most likely to alleviate or remove the very real faults of our society, (iii) to emphasize and utilize the enormous capabilities of this nation in science and technology through the applications of research and development aimed at the attainment of national objectives, and (iv) to protect and enlarge the freedom and dignity of every citizen.

I suggest a mechanism such as this National Development Act to assure that the deliberate seeking, diffusion, and application of knowledge become an integral and continuing part of policy-making in our government as it seeks to fulfill its responsibilities in attaining an improved quality of life for all our citizens.

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"Why Science Can't Save Us"

A Conversation with Dr. William O. Baker, President, Bell Telephone Laboratories

From *Forbes*, February 15, 1974, volume 113, p. 68.

The downfall of capitalism, said Karl Marx, would result when capitalists, faced with ruinous competition, tried to squeeze so much from labor as to trigger revolution. One serious flaw in that prophecy, of

course, was that it completely overlooked the impact of research and development, which generated enough new products, new materials and new methods of manufacture to keep all parties reasonably happy.

As we read the year-end predictions of mounting inflation, economic downtown and rising unemployment for the U.S. in 1974, we wondered if we had all forgotten that basic lesson.

Measured in constant dollars, the Federal Government has cut back on research spending in recent years, and private industry has barely held its own. Have we all forgotten what research wrought?

We put that question to Dr. William O. Baker, the new head of Bell Telephone Laboratories, the nation's largest and most prestigious research and development lab. We began by asking his reaction to Senator Henry Jackson's (Dem., Wash.) recent call for a new Manhattan Project to brainstorm a way out of the energy crisis (Forbes, *Dec. 15*).

"Senator Jackson has a great and active faith in science and technology, which is, of course, greatly appreciated," Baker replied. "And we subscribe to the thesis that investment in research and development has been far too low, particularly in the industrial component. We have, I think, only 1,200 research labs of *any* kind in this country, and we could support at least ten times as many, perhaps 100 times as many!—particularly in medium-sized and smaller companies.

"But, you see, one of the crucial factors in this thing is manpower—or brainpower. Even the President's minimal $10-billion energy research program, which is about half of Senator Jackson's in scope—so I suppose we call it the semi- or demi-Jackson—even *that* could use up all the scientific human resources we have. We don't have enough as it is, so just shifting them around from one place to another isn't going to do a whole lot of good."

"Industry has turned away from product creation in favor of product improvement and process research and development," Baker agreed. "What had them going in the Fifties and the Sixties were the examples of Xerox, Polaroid, nylon and the whole domain of solid state transistors, solar batteries, integrated circuits—and the computers that are derived from them—that we at Bell Labs opened up. When industry discovered that such breakthroughs just didn't happen—because they only happen a few times in a century, if they happen at all—they got fed up and fired people. In 1971, I think, they closed down six or seven of the best laboratories in the country, including the Shell Oil Fundamental Research Labs out in Emoryville, Calif., which we could certainly use now as a national resource if it still existed.

"Industry overreacted, that's all."

Doesn't U.S. industry risk losing its technological lead to countries like Japan, Germany and the Soviet Union, where research spending has been increasing much faster? In fact, isn't that already happening?

"That doesn't mean the rest of the world is better," Baker retorted. "It just means that everything is slumping. We have traced very carefully since the Fifties the course of patenting and inventing in this country, and it's going down. But it's going down all over the world—not just in the U.S."

Because of reduced spending?

"It's not easy to say it's reduced spending," said Baker. "It's more complicated than that. One of the great strategies of science and technology is to do the easiest things first. Well, we've been doing that for about 100 years now, and the problems have become much tougher.

You say we need a much larger research effort: Won't much of it need to be federally funded?

"No, I don't think so," said Baker. "You see, there's been this myth over the years—and a *dangerous* myth it is, as we've tried repeatedly to emphasize—that military and space and nuclear energy research were good for industrial and economic progress. Well, they're not really. They're economically unproductive. As a consequence, the huge figures associated with science and technology have been somewhat meaningless. We don't need another national effort of that kind; that isn't going to do us any good."

Suppose instead that the Government subsidized *industrial* research?

"I don't think industry can make a very good case for government research stimulus," Baker replied. "The companies certainly haven't done all the research they could afford to do. In fact, they tend to hold back research in some areas. But maybe they're doing the right thing from their point of view. If, as was the case until very recently, the auto companies turn out what everyone wants to buy in this country, and turn it out at pretty competitive prices, why should the Government subsidize further research? In science, as elsewhere, the most valid use of human resources results when you do the things that the market wants."

The trouble is that private industry has become disenchanted. Why?

"Now as to your concern over the technological capabilities of the Soviets: We've looked hard for any and every trace of technology from the noncapitalist, collectivist countries. We've said, 'My gosh, these people accept technology as a kind of religion. They *must* be about to add something to the world's knowledge of science and engineering.' You can't find it!

"Take fertilizer, for example, critical to their economy and to our own: They've never learned how to make it! That's almost certainly the prime reason for Khrushchev's downfall. And that's why Armand Hammer and others are playing footsie with them—they'll do *anything* to get fertilizer knowhow. In our own field of electronics and communications, they are five to ten *years* behind.

"In other areas, five or ten years ago their turbine oil drills were tops. Now not even that's the case. You know the space story. We've looked for their work in rendezvous and lunar voyages and recovery systems, and it's just *backward*.

"Recently they showed Jim Fisk, the chairman [who retired Jan. 31] of our labs, this MHD [magnetohydrodynamics] electric generating plant, which generates power by passing an ion cloud through a magnetic field. For example, Avco up in Everett, Mass. is quite good at doing this with the stack gases of power plants. Anyway, this is probably the largest demonstration plant the Soviets have ever built. Well, it turns out the darned plant doesn't work. They admit it themselves. They want help—some kind of magic advice.

"In fact, you have a very peculiar policy issue, I think, as regards some more recent threats to our high-technology industries like elec-

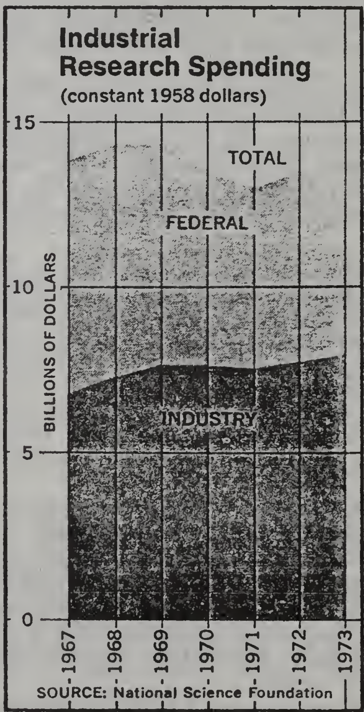
tronics. We were urged, almost ordered, to give away great amounts of our technology in the postwar period under the guise of rebuilding Europe and Japan. We're being threatened with the same thing now—that is, the Government is trying to give Russia and other collectivist countries a great deal of our technology. We're *very* concerned about this."

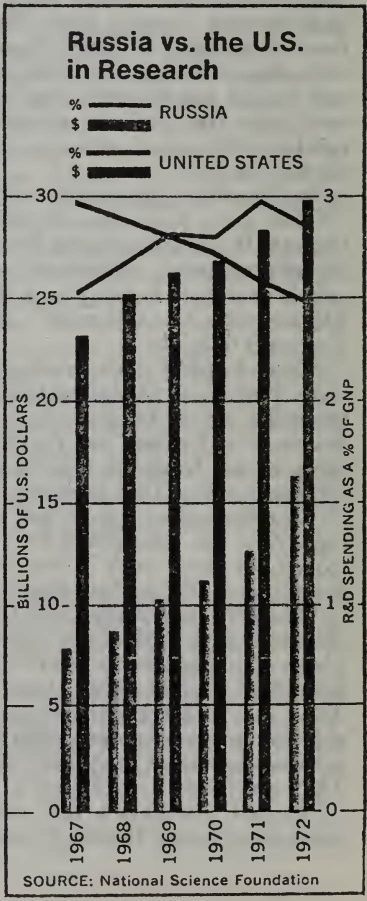
What about Japan?

We've been startled by the scarcity of innovation that has come out of that country," replied Baker. "Oh, we found a few material things, one having to do with palladium alloys—perhaps one or two other things that look pretty good—but gee, the total output is *very* low."

So what should we do? Forget about crash programs of federally funded industrial research? Let the economic markets do the allocating of money and people and pray for great scientists?

"Yes, in a way," said Baker. "I suppose it comes back to the point that cerebral energy is much more fragile than kinetic energy—so fragile, in fact, that we don't know how to assure its plentiful supply."





"Can Science and Money Solve All Our Problems—Nonsense"

Interview with Sir Bernard Lovell

[From *U.S. News and World Report,* December 1, 1975, volume 79, p. 53]

Question. Sir Bernard, some people feel modern scientific research is proving to be a curse because of the undesirable impact on our lives. Others say it is a blessing. What is your conclusion?

Answer. I think the answer is both: Modern science is a blessing—and a curse.

A very clear example of this duplicity is the use of the rocket. The rocket was developed for warlike purposes in World War II, although, of course, it already had a long history. Since that time, the rocket has been a vital part of the military arsenals of the world.

But the rocket also is used to launch scientific instruments into space, and this has enhanced our knowledge of the universe in a very remarkable way. With relatively minor changes, a rocket today can either carry warheads to destroy mankind or scientific instruments for the investigation of the remote parts of the universe.

This is a clear example in the physical sciences, but other scientists, almost without exception, can give similar examples of this thin dividing line between the good and evil results of science.

On the other hand, there are more-fundamental issues in modern scientific research which I believe to be altogether a blessing. Many discoveries have presented us with very great intellectual challenges. For example, in my own field of astronomy we are now faced with problems concerning the nature and origin of the universe as great as man has ever confronted.

If you believe, as I do, that the attempt to understand our environment and our place in the universe is one of the basic driving forces in the advancement of civilization, then these aspects of modern science are altogether good and, indeed, are an essential part of man's progress.

Question. Yet you said recently that "the simple belief in automatic material progress by mean of scientific discovery and application is a tragic myth of our age." Exactly what do you mean?

Answer. In the last two decades, the governments of the great powers have been persuaded that a massive financial investment in what is called "science" will automatically produce goods and machines which are valuable to the state in a purely material sense. That is, they will improve industry, improve the economy, and generally boost the power of the state both in civil and military ways.

This is nonsense.

It's a very odd thing, but in many ways we are suffering from the immense value of science to the Allies in the second World War. Science and scientists emerged from that conflict very highly regarded by our governments. Scientists certainly did play an invaluable role in defeating the common enemy. But this led on the peculiar belief that if you can put enough scientists and enough money into the development of science you can solve any problem.

The fact that science in the war was integrated in a massive and brilliantly organized military operation was disregarded, and the nec-

essary social and organizational frameworks in which science might have been able to solve postwar problems did not emerge.

You cannot invest in science and expect to get results automatically, even in fundamental research and certainly not in the applied technological aspects. Science is not a magic wand you can wave and turn a poor country into a rich one. As soon as you try, a host of other problems arise which have nothing to do with science and cannot be solved by scientists or the methods of science.

Question. Has such an attempt been made?

Answer. Of course—look at the Soviet Union and the tragic failure of the grain harvests.

But perhaps the clearest example is here in the United Kingdom. In 1963, Harold Wilson [then the leader of the Labor Party and now Prime Minister] spoke of the "new" Britain that would be forged in the "white heat" of a technological revolution to be initiated when the socialist party gained control.

A year later they did gain power and created a Ministry of Technology. This department commanded some of the best scientific brains in Britain. But as soon as it began to apply modern concepts of science and technology to solving certain industrial problems, there were massive social reactions and labor problems to which neither the scientists nor the Ministry had answers.

The point is that science and technology is only one part—although an essential one—of the process of improving society. And it is rather an expensive cog in a very big and complex machine. The failure of the political body to realize this has led to a regrettable reaction against science and scientists from which the Western world is still suffering.

Question. What form is this reaction taking?

Answer. Well, the most obvious example is the severe cut in the budgets for scientific research in a number of countries.

In the United Kingdom, for instance, a desperate situation has been worsened by the economic squeeze in the universities, which is rapidly turning them into teaching factories instead of centers of intellectual excellence and fundamental research.

In the United States, a fierce reaction occurred after the accomplishment of the near scientific and technological miracle of landing men on the moon and getting them back again.

The erosion of the faith and hopes which inspired these enterprises, and the rapidity with which the budgets were slashed, will surely emerge as one of the great ironies of the twentieth century.

Question. What lies behind this kind of reaction?

Answer. Quite simply, the explosion of the myth that science can solve all problems given sufficient resources, and the realization by politicians that the promises made in the name of science 10 or 15 years ago have not been realized. Sometimes this has been for technical reasons; more often because of a failure to understand that science is only one part of a far greater and more complex social problem.

Question. Is there some truth in the view that science has created as many problems as it has solved?

Answer. That is a very disputable point. I think we are living in an epoch which renders it extremely difficult to make any final judgment.

In many aspects of research—and I am thinking particularly of those whose technological application may lead to the contamination

of the environment and the atmosphere—I would say the situation needs watching with some anxiety. Many of these contemporary developments can be of immense benefit to mankind, but can also be very harmful either through neglect or sheer accident.

However, when I think of my childhood and the state of the world then and the facilities which I had as I grew up compared with those my children and grandchildren enjoy now, it is undeniable that so far science and technology have been of immense benefit to the world.

Whether this situation will continue is a critical question. The precarious balance of power in the world is maintained by a weapon whose potential for destruction defies the imagination. I find it impossible to escape a feeling of deep anxiety.

Question. Does the recent move by genetic engineers to apply self-imposed controls to their research point one way ahead? Should other scientific disciplines follow this lead?

Answer. I hesitate to give opinions outside my own profession. But when the dangers of research can be so clearly enunciated as they are in this case, I do think other scientists would do well to take note.

The trouble is that it's not always so easy to pinpoint the dangers far enough in advance. Take atomic weapons, for example. One would have had to stop the researches into atomic structure half a century ago. Neither Ernest Rutherford nor any other responsible scientist at that time foresaw any practical outcome of these researches.

Question. In view of these dilemmas, should governments assume closer control of scientific research?

Answer. Almost without people realizing it, science has already become very highly centralized.

This first happened in the Soviet Union—which, incidentally, I have just revisited after a 12-year gap. Here you see the most-immense changes in the sciences and in the consequences of science because Soviet science has been concentrated on specific issues. Some of their research, particularly in the astronautics and aeronautics field, is absolutely first class. Other areas are not so good. I am simultaneously impressed and alarmed by the great power which this kind of direction of science gives to a state.

In a more subtle and perhaps more gentlemanly sense, the same process is evident in the Western world because science is now being centralized in government financed organizations. It's an extremely difficult situation, arising because the high cost of research leads to the demand for public accountability and centralized control.

Question. Why are you so concerned about this trend? Doesn't it make research more cost-effective?

Answer. On the contrary, I fear that growing centralization and costs of science lead to inefficiency.

It is becoming exceedingly difficult, for instance, to do entirely new kinds of science. Centralization means you have to have a massive supportive paperwork organization. It means the best brains often are wrapped up in doing an enormous amount of administrative work.

Sometimes it seems one must know in advance what scientific results one is going to get before one can try to obtain the large amounts of money necessary for modern science. The state is reluctant to allow individuals or universities to control the money. So there has to be a series of intermediate organizations responsible to the central gov-

ernment. That's the difficulty: The freedom which is an essential prerequisite of the best research disappears as the budgets increase.

Question. Should scientists themselves take the responsibility for the emphasis and goal of their work?

Answer. Yes. But the scientist today can only work through the complex machinery of government. The power of this machine is so very great that it can change the individual instead of the individual changing it.

Question. And this is largely because modern scientific research is so costly—

Answer. Yes, I think that is so. We have evolved into an age where a large majority of scientists believe that in order to do any worthwhile research they require multibillion-dollar equipment. They often do. But at the same time there are dangers, because equipment like this inevitably directs one along a certain line of research. One always tends to concentrate on areas that stretch the technique and available equipment to the limits. There is therefore a tendency to neglect other areas which may be important.

The classic example occurred in 1967 in my own profession with the discovery of the pulsar [a celestial source of pulsating radio waves]. These neutron stars were not discovered by those of us with large and expensive telescopes and computers. They were discovered accidentally by a girl research student studying a pen-and-ink chart produced by a relatively cheap radio telescope which was designed, and in use, for an entirely different purpose.

Question. Is there any way that science can put the genie of the atomic bomb back in the bottle, so to speak?

Answer. I think it would be fatal to attempt to do that. I think if you tried you would stifle the advance of civilization. It may be that civilization can be destroyed because of the applications of science, but that is an entirely different matter.

One shouldn't try to halt the advance of knowledge, whether it be in science or any other discipline. The search for knowledge and for a deeper understanding of man and his place in the cosmos is a vital necessity for human advancement.

Question. Would you say that we are in danger of learning more than is "good" for us?

Answer. Not in any intellectual sense. I think that one of the most extraordinary things about the discoveries of modern science is that we have not achieved a completely scientific description of the universe. On the contrary these investigations of the distant regions of time and space have revealed great difficulties which seem to confront the laws of physics with problems which may be insoluble.

Many eminent scientists today are puzzled by this problem. Indeed, it may be asked whether there is a limit to scientific explanation because of the involvement of man himself in the object of his investigation.

Question. Is that the only line of inquiry?

Answer. No. If you believe that science can eventually explain everything then you search for solutions which may lie beyond our present understanding of fundamental physical theory.

You find this attitude exemplified by the powerful Soviet school of theory. For example, they acknowledge the problem about the concept of the beginning of the universe from a state of infinite density and infinitesimal dimensions. But they believe these infinities are introduced by an inadequacy of physical theory which they are trying to overcome.

Question. Does this mean that Soviet scientists are less concerned with the sort of moral dilemmas that worry you.

Answer. I don't think they recognize their existence. We all owe an allegiance to the state in which we live and work.

I'm not expressing any criticism. It's just a fact that few scientists—East or West—worry over the moralities of their work. The majority are in science as a profession by which they earn their living.

Question. Have you changed your mind about the impact of science on society as you have learned more?

Answer. Oh, yes—of course I've changed. Science in the late 1920s and 1930s seemed to me to be tremendously powerful. I was an impressionable young man being educated during dramatic phases of discoveries in astronomy and physics.

Question. What contributed to this change?

Answer. Two factors, really:

First, in the sphere of knowledge, I don't believe any more that science is all-powerful. Indeed, I now think that the belief that it is so is positively dangerous, and that we have deluded ourselves that through science we find the only avenue to true understanding about nature and the universe.

The lifeline of civilization is the pursuit of understanding. Science is a vital and powerful human activity in this pursuit. But the search for scientific understanding does not embrace the totality of human purpose. The material and scientific searches are embraced in the transcendent values of human existence and purpose. The disregard of this world view is creating severe problems for contemporary society.

Second, in the sphere of material welfare, I no longer believe science is all-powerful for reasons I mentioned earlier. The massive application of science and technology to national problems and to easing the tragic human conditions in the "third world" has not been conspicuously successful.

Science and technology are vital parts—but only parts—of a complex social and political situation. Without this recognition, their application may well increase the gravity of the problems they are designed to solve.

Question. Are Western societies beginning to accept the limitations you are talking about?

Answer. It's agonizing to me that there is so little understanding and agreement about the correct and *separate* functions of science and technology in modern society.

We are at the nadir of faith in science as a great enterprise. But the vision will return.

I remain essentially optimistic.

"The Economic and Technological Implications of 3 Percent Growth"

By Robert L. Heilbroner

[From *Technology and Culture,* October 1968, volume 9, p. 570]

A milestone of potentially great significance was passed on December 29, 1967, when the first joint session of the Society for the History of Technology and the American Economic Association was held at the annual meeting of the latter in Washington, D.C. As the chairman of the initial session, I remarked in my opening benediction that technology and economics had been living in a common-law marriage since the days of Adam Smith, although, curiously, each side of the union seemed honor bound not to mention the name of the other in public. Alfred Marshall and J. M. Keynes, for example, the greatest economists of their respective times, both propounded theories of economic society that began with the premise that the quantity and quality of capital—that is, its technology—were to be taken as "given" and changeless, while the greatest technological minds of the century—its practicing engineers or its philosophers of science—designed their machines or propounded their explanations of the dynamics of science and technology as if the economic milieu were "given" and changeless. Yet in fact everyone knew that the single most dynamic force within the economic system was its changing frontier of technical possibilities and that the single most important influence on the rate and direction of change of that frontier was the constraints imposed by the economic relationships of the system. Hence it was about time for the illicit relationship to be brought into the open and legitimized. Whether or not they would settle down and live happily ever after, I remarked, it was certaily time that economics and technology got married.

The theme of the initial joint session was "The Economic and Technological Implications of 3 Percent Growth." "Was a continuance of growth along the historic trajectory of the past possible for another century?" asked the organizer of the initial session. The "answer" to such a question, he pointed out, depends in part on long-run relationships among the economic variables of the system. Rather than attempt to cover the huge complexities of each side of the question in their short papers, he suggested that the spokesman for technology concentrate on the problem of "running out of technological impetus," while the spokesman for economics should pay attention to the possibility of "running out of consumer demand."

Michael Michaelis (Arthur D. Little Co.) approached the first question boldly. There was, he stated, little or no reason to believe that the supply of technical stimulus would decline over the future; indeed, Michaelis went so far as to declare that "we now have, or know how to get, all the technical knowledge needed to satisfy foreseeable human wants." The problem, he suggested, was not the generation of new knowledge but its *application*—bringing the virtually illimitable capabilities of scientific and technical know-how to bear on the social and economic framework. Continued growth along the historic path will hinge, therefore, on new breakthroughs in science and technology less

than on the continued or accelerated translation of steadily accumulating knowledge in useful form.

Michaelis took the position that such a steady application of technology would not take place spontaneously but would require the more or less deliberate adoption of a systems-analysis approach, in which the piecemeal, catch-as-catch-can introduction of technology characteristic of a market economy is replaced by a planned and designed approach in which scientific and technological resources are marshaled from the beginning toward the solution of a designated problem. He compared in this regard the long-drawn-out and haphazard "solution" to the social problem of keeping clothes clean—a solution that has resulted in uneven and unco-ordinated advances in textile techniques and laundry and drycleaning technology, finally to culminate in the new concept of disposable clothes—with the unified efforts that is providing in an extraordinarily short time the technical capability to reach the moon. Yet each effort—the cleaning of clothes and the reaching of the moon—has cost about the same amount of money.

Can such a systems approach be applied to technology in general? Michaelis was not clear in this regard. He was aware of the difficulties of introducing new systems, especially when these interfere with settled routine and vested interests. Yet in the application of systems approaches to the solution of educational problems (building and design of parts of the California school system), he saw evidence that the economic system may yet adapt itself to this new social technique. In the end, Michaelis' paper was perhaps more of a prescription than a prediction. It was a call for social change along "technocratic," or at least socially functional, lines as the necessary condition for applying the requisite force of science and technology to the economic mechanism.

In a comment on Michaelis' paper, Joseph L. Fisher (Resources for the Future) strongly endorsed the systems approach, particularly with reference to ecological problems, such as water or air pollution. Fisher shared Michaelis' qualified optimism as to the availability of the requisite scientific knowledge, to which he added a word of reassurance as to the availability of basic resources adequate to maintain continued advance—with a strong caveat as regards the underdeveloped world—provided that these resources are managed in accordance with intelligent policies of conservation, etc.

The economic aspects of continued expansion at the historic rate present the question of whether we will be able to consume the flow of goods inherent in such a projection. In his paper, Henry Villard (City College, N.Y.) extrapolated the trend of consumption growth into the future and concluded that at least within the span of another century there is no reason to believe that we could not cope with the problems it raised.

Assuming that the growth rates of the past are maintained—in other words, that the technical or otherwise-provided impetus of the system remains—and that the work week declines to twenty-six hours a week for forty-six weeks per year, in line with its decline over the past century, Villard arrived at a representative income per member of the labor force of $36,000 for the year 2065. Judging by the behavior of consumer units who enjoy this income today, he claimed there is little danger of "satiation" for the individual buyer. Rather, the ques-

tion then arises as to whether the *collective* consumption expenditure of this amount (in terms of today's prices) presents problems or contradictions.

Villard admitted that the composition of consumption items in a world of generalized wealth might perforce differ from that of a world of skewed wealth. "If 1 percent of beef production is filet mignon * * * it [will] obviously be impossible—short of an unlikely re-engineering of the steer or an immensely wasteful production of meat—to provide the average consumer with filet mignon in 2065." Thus, as in the past the rise of real incomes of consumers, quite apart from any redistribution of income among consumers, acts to change the composition of the "market basket" of goods enjoyed by the formerly rich.

This difficulty is not, however, a crucial one, although it may present its psychological problems as well as index-number problems in measuring the "real" change in well-being. The crux of the question is whether mass consumption at the projected level is feasible. This hinges, Villard believed, on the *types of consumption* that such a society wishes to enjoy. That is, some kinds of consumption, designated by Villard as "neutral," do not affect the environment—for example, the kinds of food I eat (although if all consumers eat the same kinds of food, there may be a substantial environmental effect in terms of different factor inputs required to produce different food outputs). Other kinds of consumption, however, exert significant and often "diverse" environmental effects—for example, the ecological impact of the automobile, the airplane, etc. Here we rapidly come up against certain inelasticities, especially those of sheer space, that can lead to very serious social impasses.

The problem, in a word, is that neighborhood effects tend to rise with the square, and not the absolute, number of the population and, by extension, by an exponential function of the level of commodity use.[1] Nonetheless, Villard did not find the problem unmanageable, provided that we "devote a significant part of the increase in real income to the reduction of the adverse repercussions in question." He concluded, however, with a pessimistic warning that this sanguine conclusion does not apply to the world at large, where an impending population crisis does raise problems of presently unmanageable dimensions.

Discussing Villard's paper, Ben S. Seligman (University of Massachusetts) emphasized strongly the potentially disruptive externalities of uncontrolled consumption (stressing the Baumol effect). However, Seligman focused his attention mainly on the productive rather than the consumptive side of the growth extrapolation and pointed to the potentially dislocative effects of the redirection of manpower away from the highly mechanized sectors in which technology enters toward the low-productivity service areas. This may result in a failure of incomes to match productivity, at least for large sections of the population whose ability to maintain living standards will be accordingly impaired. Seligman seemed to envisage a dual-sector economy arising from the uneven entry of technical progress, with a small, perhaps shrinking, nucleus of advanced technology and high-income work and a larger and expanding periphery of low-productivity,

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[1] See W. J. Baumol, "Macroeconomics of Unbalanced Growth," "American Economic Review" (June 1967), p. 424.

relatively lower-paid occupations, and he saw in this duality serious problems for the continued steady expansion of output.

Summing up all papers, Victor Fuchs (National Bureau of Economic Research) found cause for cheer in the avoidance by all participants of the extremes to which long-run projections are prone-extremes of science-fiction abundance on the one hand and of resource exhaustion or vast technological displacement on the other. The problems of continued growth can be encompassed without recourse to such apocalyptic visions, he suggested. And yet a crucial problem remains in the values for which growth is attained and to which it is put. The essential questions to which a study of long-term trends directs one are therefore the social, political, and moral aspects of existence rather than purely technical obstacles of either science or economics proper. The real impact of continued economic growth and technological advance, Fuchs maintained, will be found in such areas as the obsolescence of the nation-state, the family, and the value systems of society and in the need to discover new viable forms of institutional structure and spiritual belief.

Inevitably in papers of the brevity enforced by the occasion, problems are easier to identify than to explore. The session served well to highlight many issues connected with long-term growth, in particular the need for the social and political changes required to produce sustained technical change and to absorb it. Indeed, if any single point of agreement emerged, without prior consultation, it was the need for new public (or public-private) social arrangements, both to guide technology and to restrain the private uses of output. In a very real sense, then, both from the technological and economic sides, a convergence on the problem of social adaptation and social change emerges as the central theme of this first attempt to extrapolate the parameters of continued expansion.

Yet. other questions remain. to be dealt with, hopefully, by future contributors to joint SHOT–AEA meetings. Three of them are suggested herewith:

1. The tremendous expansion of scientific and technological activity of modern times has *not* been accompanied by a change in the over-all rate of growth, at least in America since the Civil War. This suggests that the relation between scientific advance and economic expansion is more complex than has yet been recognized. If an "exponential" growth of science has resulted in no more than a continuance of the given historical rate of advance, this suggests that future advance will require an impossible rate of scientific application—or that economic growth rests on something other than scientific progress.

2. The increase in the standard of living brings not only problems of externalities but changes in the motivational foundation of the market system. Increased wealth per capita seems likely to effect the acquisitive (maximizing) behavior on which the behavior of the system is assumed to rest, whether in terms of lowering the marginal utility of the carrot or padding the marginal disutility of the stick.

3. There remains the problem of how social routine and vested interests can be overcome to cope with new priorities and problems that find their roots in the socially disruptive effects of technology and a changed economic environment alike.

All these problems, and no doubt many more, require for their analysis a joint consideration of both technical possibilities and economic relationships—of what can be done within the constraints of nature and the human capability to master nature and of what may be done within the existing network of behavioral and social (and ideological) relationships to master technology. The first SHOT–AEA meeting has bravely sent out its first reconnaissance parties into a largely unexplored terrain. It remains for subsequent explorers to claim new portions of this vast terrain in the name of human understanding.

"A Review of the Relationship Between Research and Development and Economic Growth/Productivity" Papers and Proceedings of a Colloquium National Science Foundation—Introduction and Summary

By Leonard Lederman

[National Science Foundation, December, 1971 (72–303)]

I. INTRODUCTION

The question of what is known about the relationship between research and development and economic growth/productivity is long standing. Both the research and development community and the economics profession have been concerned with the subject for sometime. A relatively small number of economists have conducted research on the subject, beginning in about the mid-1950's. The National Science Foundation has a continuing interest in the subject both as a significant funder of research and as a Government agency with major science-policy responsibilities. Accordingly, NSF has sponsored some of the economic research on the relationship between research and development and economic growth/productivity.

II. SCOPE AND PURPOSE

In mid-September 1970, the Office of Management and Budget asked NSF to put together a quick review of the subject. NSF translated this request into the following questions:

(a) What do we know about the relationship between R&D and economic growth/productivity?

(b) How good is the current state-of-the-art on the subject?

(c) Are we in a position to make any judgments concerning whether are United States is under or overinvesting in R&D purely from the economic growth/productivity aspect?

(d) What might be the next logical steps in furthering our knowledge of the relationship so as to reduce the area of judgment?

This is not an effort to add substantively to knowledge of the subject, but rather a modest effort to review what is known about the economic aspects of the subject and what the existing knowledge "adds up to." Before doing so, however, two very important points should be stated explicitly:

"First, by focusing attention on the economic effects of R&D, we are not implying that only these effects of R&D are important or relevant. On the contrary, increased knowledge is clearly of great importance above and beyond its strictly economic benefits. However, our assign-

ment in this paper was to confine our attention strictly to economic matters. Second, by looking at our Nation's rate of economic growth and productivity increase, we are not assuming explicitly or implicitly, that economic growth is, in some simple sense, what public policy should attempt to maximize. Clearly, the desirability of a particular growth rate depends on the way it is achieved, how the extra production is distributed, how growth is measured, and many other things." (Mansfield)

III. APPROACH

To accomplish the purpose set forth above, the NSF commissioned reviews of the subject from leading researchers in this field. The papers that follow are the output of this effort, and are necessarily limited by the scope described, as well as by constraints which each contributor faced in terms of his own time and the short deadline placed upon him. NSF is indebted to the contributors for producing this material under such circumstances and asks that the reader view the output with an understanding of the scope and limitations described. As a followup to the preparation and limited distribution of this material, NSF plans a symposium during which readers and the authors can exchange ideas on the subject. The preliminary papers contained herein (perhaps modified) and proceedings of the symposium will then be considered for publication.

IV. CONTENTS

The first paper, by Professor Stewart, is *A Summary of the State-of-the Art on the Relationship Between R&D and Economic Growth/Productivity.* It describes the methodologies employed, the estimates derived, and the results of research on the subject. Professor Stewart's paper reviews the most pertinent literature reporting research dealing with the total U.S. economy (macro), the industry level, and the firm level (micro). It seeks to answer the first question: "What do we know about the relationship between R&D and economic growth/productivity?"

Professor Mansfield's paper on *The Contribution of Research and Development to Economic Growth in the United States* begins with a discussion of the second question: "How good is the current state-of-the-art on the subject?" including a discussion of the "Fundamental Problems of Measurement" and an "Evaluation of the Evidence." He provides his views on the third question (Are we in a position to make any judgments concerning whether the United States is under or over-investing in R&D purely from the economic growth/productivity aspect?) and presents ideas on the fourth question (What might be the next logical steps in furthering our knowledge of the relationship so as to reduce the area of judgment?)

Professor Fellner's paper entitled *The Progress-Generating Sector's Claim to High Priority* discusses the questions raised from the perspective of his research utilizing the residual methodology in estimating the macro economic contribution of progress-generating activities. He summarizes the results he has achieved which were most recently reported in his Presidential address delivered at the eighty-second meeting of the American Economic Association on December 29, 1969. Professor Fellner's paper discusses: the problem estimates of average

rates of return, obsolescence, marginal analysis, reduction in research funding, and comments on worthwhile projects.

The fourth contribution, by Professor Griliches, is *A Memorandum on Research and Growth.* Professor Griliches discusses the evidence for a positive return from R&D, arguments on the underinvestment questions, some hunches and ideas concerning the allocation of resources, and suggestions for research topics.

V. SUMMARY

The following is a summary of the views of the contributors with regard to the questions raised under the Scope and Purpose section. The summary with regard to each question is organized first to present briefly the major overriding conclusion, and then to present materials from the papers which provide support for, and extension of, the major points, including the range of views.

(a) What do we know about the relationship between R&D and economic growth/productivity?

Although what we know about the relationship between R&D and economic growth/productivity is limited, all available evidence indicates that R&D is an important contributor to economic growth and productivity. Research to date seeking to measure this relationship (at the level of the firm, the industry, and the whole economy) points in a single direction—the contribution of R&D to economic growth/productivity is positive, significant and high.

* * *

Various studies have attempted to establish different things. First they have examined the correlation between R&D time series (or R&D aggreated over a number of years) and time series of productivity increase (or productivity increase over a number of years) . . . A positive and significant correlation has usually been found . . . Some have calculated rates of return and marginal rates of return on investment in R&D. These have usually been quite high, but they vary widely. (Stewart)

* * *

* * * it is clear that the current state-of-the-art in this area is not strong enough to permit definitive estimation of these relationships. Nonetheless, although the results are subject to considerable error, they establish certain broad conclusions. In particular, existing econometric studies do provide reasonably persuasive evidence that R&D has a significant effect on the rate of productivity increase in the industries and time periods that have been studied. (Mansfield)

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* * * these studies rely on the results of several econometric investigations that indicate that, for the industries and fields under investigation, the marginal rate of return from an investment in research and development has been very high. (Mansfield)

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All reasonable ways of looking at the matter lead to the conclusion that the rates of return are very high as compared to usual estimates of rates of return on capital formation. (Fellner)

Investment in research, both private and public, has clearly been one of the major sources of growth in output per man in this century. It has been a good investment both in the sense that it yielded a positive rate of return, and in the sense that this rate of return has been as good and often better than the rate of return on other private and public investments. The evidence for these statements is scattered; much of it is secondhand; but it is still quite strong. (Griliches)

* * *

The actual estimates of contribution and rate of return can only be understood in terms of what level is under study (e.g., individual innovations, the firm, the industry, the economy as a whole), what is being measured (e.g., contribution to economic growth, to national productivity, to the productivity of an individual industry or firm), and what methodology is employed in making the measurement (e.g., direct econometric measures, aggregate residual measures). Accordingly, the reader must read the full context in which such estimates are provided. The following are the major estimates provided with a reference to the text which is necessary for proper understanding.

* * *

Turning to manufacturing, Mansfield and Minasian estimated the marginal rate of return from R&D in the chemical and petroleum industries. Mansfield's results indicated that the marginal rate of return was about 40 percent or more in the petroleum industry, and about 30 percent in the chemical industry if technological change was capital embodied (but much less if it was disembodied). Minasian's results indicated about a 50-percent marginal rate of return on investment in R&D in the chemical industry. In addition, Mansfield provided some evidence that the marginal rate of return seemed relatively high (15 percent or more) in the food, apparel, and furniture industries. (Mansfield)

* * *

As explained on pp. 19–20 of my article in the March 1970, issue of the AER (AEA presidential address), the average rates of return calculated in the manner described above fall in the range between 31 percent and 55 percent for 1966. (Fellner)

* * *

This evidence is of three kinds: individual invention returns calculation, industry studies, and aggregate residual attribution calculations . . . The internal rates of return implied by these estimates of individual inventions are quite high (10 to 50 percent per annum), even though they are usually based on conservative assumptions . . .
While each of these (industry) studies is subject to a variety of separate reservations, together they all point to relatively high (30–50 percent) rates of return on average to both public and private investments in research. (Griliches)

* * *

Thus, without knowing definitively what is the relation between R&D and productivity change, we can safely conclude that our existing measures of productivity change understate the contribution of R&D. (Stewart)

* * *

(b) How good is the current state-of-the-art on the subject?

While there are differences concerning the adequacy of present research findings, these seem to affect the degree of confidence the contributors place on the estimates rather than the direction and rough magnitude of the estimates. The current state-of-the-art is not strong enough to permit definitive estimates of the contribution of R&D to economic growth/productivity and results are subject to considerable error. Nonetheless, the results cited in the papers lead each of the authors to the conclusions stated above.

* * *

By the standards of the scientific community one cannot be strongly assertive about some of the analytical results to be presented, in particular about the underallocation of resources to the progress-generating activities. The conclusions will be formulated accordingly. However, in most practical decision problems it is necessary to rely on somewhat inconclusive evidence, and the indications derived from the materials here surveyed are strong enough to be considered analogous to indications on the basis of which action is indeed taken in many real-life situations. (Fellner)

* * *

In conclusion, technological change has certainly contributed in a very important way to economic growth in the United States. Although existing studies have not been able to estimate this contribution with great accuracy, they have certainly indicated that this contribution has been large. Moreover, although econometric studies of the relationship between R&D and productivity increase have been subject to many limitations, they provide reasonably persuasive evidence that R&D has an impotrant effect on productivity increase in the industries and time periods that have been studied. (Mansfield)

* * *

The research results reported above are subject to a number of important limitations of at least two major kinds:

1. Limitations concerning the specific findings and specific methodologies employed. Examples include: the difficulty of going from contributions of technological change to the specific contribution of R&D; the problem of estimating the time lag between R&D investment and economic impact; difficulty in deflating R&D expenditures for price changes. These are best considered simultaneously with the findings, and the references provided above (a) should enable the reader to do this.

2. Limitations that are more general and affect much, if not all, of the work to date. Examples include: difficulty of isolating any one factor (i.e., R&D) from the complex interaction of factors contributing to economic growth/productivity; measurements that inadequately reflect quality changes; positive correlations that do not necessarily imply causation. The following excerpts from the papers summarize this second kind of limitation.

* * *

The state-of-the-art is least satisfactory in measuring the relationship between R&D and productivity gain. This is true for two reasons: 1. R&D (or economic resources devoted to the advancement of pro-

ductivity-relevant technological knowledge) is not the only source of productivity growth; and 2. R&D and other sources of growth are interdependent.

Other factors in productivity gain which have been stressed include organizational and managerial progress and economies of scale and urbanization. But their contribution is no more readily quantified than that of R&D. The major difficulty seems to be interdependence. (Stewart)

* * *

First, the measured rates of growth of output on which these estimates are based suffer from a very important defect, particularly for present purposes, because, to a large extent, they fail to give proper credit and weight to improvements in the quality of goods and services produced, and these improvements are an important result of research and development. For example, the growth rate would have been the same whether antibiotics were developed or not, or whether we devoted the resources used to reach the moon to public works. In general, only those changes in technology that reduce the costs of end products already in existence have an effect on measured economic growth. Unfortunately, the measured growth of national income fails to register or indicate the effects on consumer welfare of the increased spectrum of choice arising from the introduction of new products. (Mansfield)

* * *

There are difficulties caused by the fact that much of the Nation's R&D is devoted to defense and space purposes. For example, some observers note the tremendous increase in R&D expenditures in the post-war period and conclude that, because productivity has not risen much faster in the United States than before the war, the effect of R&D on economic growth must be very small. What these observers forget is that the bulk of the Nation's R&D expenditures has been devoted to defense and space objectives and that the contribution of such expenditures to economic growth may have been limited. (Mansfield)

* * *

Even if one were sure that the R&D figures were reliable, there would still be the possibility of spurious correlation. Firms and industries that spend relatively large amounts on research and development may tend to have managements that are relatively progressive and forward-looking. To what extent is the observed relationship between R&D and productivity increase due to this factor rather than to R&D? Obviously, this is difficult to answer since the quality of management is very difficult to measure. Nonetheless, most investigators seem to feel that only a small part of the observed relationship is due to spurious correlation of this sort. (Mansfield)

* * *

A large percentage of the R&D carried out by many industries is directed at productivity increase in other industries. Consequently, relationships between R&D in an industry or firm and productivity increase in the same industry or firm catch only part of the effects of R&D. * * * Also, the estimates that are obtained depend on the extent of the lag between the time when R&D is carried out and the time when

the effects of R&D show up in productivity indexes. Clearly, this lag is often substantial. Unfortunately, the models on which these estimates are based often make very crude assumptions concerning the length of the lag. (Mansfield)

* * *

Studies of national productivity vary in completeness. Some limit themselves to the private nonfarm economy; others cover the total private market economy; others include government as well. The reason for excluding government in some studies is the lack of any measure of government output, and therefore of productivity change. As a result, studies including government count it at cost, probably understating productivity gain for the economy as a whole. Some studies assume a productivity gain in government equal to that in the service sectors (not all of which have measurable output and therefore measured productivity change). (Stewart)

* * *

Technological progress has harmful as well as beneficial byproducts and the harmful byproducts also express themselves largely in nonmarket results which may be regarded as negative nonmarket values. These negative items evade rough numerical appraisal by rule of thumb, and this is partly because it is not clear to what extent they are associated with the technological progress in its various phases. (Fellner)

* * *

The rate of utlization of plant and equipment varies substantially as a percentage of capacity. This results in cyclical variation in capital (and total factor) productivity. But if there is a long run trend toward fuller utilization of capacity, total factor productivity increase will be slightly higher than it would have been for a given level of utilization. (Stewart)

(c) Are we in a position to make any judgments concerning whether the United States is under or overinvesting in R&D purely from the economic growth/productivity aspect?

The authors agree that, based upon the evidence, good judgment would lead to the conclusion that the U.S. is probably underinvesting in civilian sector R&D from a purely economic growth/productivity point of view. However, nothing can be said, based upon this conclusion, as to where particular R&D investments should be made. What this judgment means is that there is good reason to expect that a well diversified incremental R&D investment will result in high payoffs similar in magnitude to those of the past.

Turning to the adequacy of the Nation's investment in research and development, there is too little evidence to support a very confident judgment as to whether or not we are underinvesting in certain types of research and development. However, practically all of the studies addressed to this question seem to conclude, with varying degrees of confidence, that we may be underinvesting in particular types of R&D in the civilian sector of the economy, and the estimated marginal rates of return from certain types of civilian research and development seem very high. (Mansfield)

Reasonable estimates lead to the conclusion that the returns are very high in the American economy. A good case can therefore be made for

increasing the weight of progress-generating inputs in the economy as a whole. Of late we have been moving more in the opposite direction. (Fellner)

Most economists, if queried, would assert that there is underinvestment in research by private firms because much of its product is not capturable (appropriable) by the private firm. (Griliches)

The first proposition is that, because the results of research are often of little direct value to the sponsoring firm but of great value to other firms, there is good reason to believe that, left to its own devices, the market would allocate too few resources to R&D—and that the shortfall would be particularly great at the more basic end of the R&D spectrum * * *

The second proposition is that, because research and development is risky for the individual firm, there is good reason to believe that the market, left to its own devices, would allocate too few resources to R&D. Of course, the risk to the individual investor in R&D is greater than the risk to society, since the results of the R&D may be useful to someone else, not to himself, and he may be unable to obtain from the user the full value of the information. Because the economic system has limited and imperfect ways to shift risks, there would be an underinvestment in R&D. For this reason too, one would expect underinvestment to be greatest at the more basic end of the R&D spectrum. (Mansfield)

(d) What might be the next logical steps in furthering our knowledge of the relationship so as to reduce the area of judgment?

All contributors agree that many of the limitations discussed in the papers are a result of the relatively small amount of research attention given to this subject. No one is satisfied with the current state-of-the-art and priorities in this respect are suggested in the papers.

"Science, Invention and Economic Growth"

By Nathan Rosenberg [1]

[From *The Economic Journal,* March 1974, volume 84, p. 90]

I

Not too many years ago most economists were content to treat the process of technological change as an exogenous variable. Technological change—and the underlying body of growing scientific knowledge upon which it drew—was regarded as moving along according to certain internal processes or laws of its own, in any case independently of economic forces. Intermittently, technological changes were introduced and adopted in economic activity, at which point the economic *consequences* of inventive activity were regarded as interesting and important—both for the contribution to long-term economic growth and to short-term cyclical instability. Schumpeter, for example, saw the engine of capitalist development as residing in this innovative process in the long run, and at the same time he developed a business cycle theory which centered upon the manner in which the capitalist economy absorbs and digests its innovations. In Schumpeter's model,

[1] The author is grateful to Professors S. Engerman, W. B. Reddaway and E. Smolensky, and to an anonymous referee for their helpful comments on earlier drafts of this paper. They are, however, accorded the usual absolution for all remaining deficiencies.

exogenous technological changes stimulated investment expenditures, the variations of which, in turn, generated cyclical instability.

In the years after the Second World War the economist's attitude gradually changed. The vast expenditures on Research and Development made it increasingly obvious that inventive activity was—or could be made to be—responsive to economic needs (or even to non-economic needs if such needs received sufficient financial support). Clearly much of the search activity of R and D was highly purposive: business firms were looking for new techniques in specific categories of products, they spent much money upon this search, and they were sometimes highly successful. Similarly, government agencies had long directed research into specific problem areas and in some cases had achieved conspicuous successes—as in agriculture.

In addition, the growth of interest in technological change after the Second World War was closely connected with the increasing concern over the prospects for economic growth in underdeveloped countries. When economists turned their attention to this range of problems, they brought with them an intellectual apparatus which placed overwhelming emphasis upon the role of saving and the growth in the stock of capital goods as the engine of economic growth. But it soon became clear that long-term economic growth had taken place at rates far beyond what could plausibly be accounted for by mere growth in the supply of conventionally-measured inputs. It became increasingly obvious that economic growth could not be adequately understood in terms of the use of more and more physical inputs, but rather that it had to be understood in terms of learning to use inputs more productively. With this realisation came, of course, a renewed interest in technological change as the source of rising resource productivity.

The growing interest in the role of technological change as a contributor to economic growth led to a considerable amount of empirical research on technological change, particularly in two areas: (1) attempting to quantify the contribution of technological change to the growth in long-term resource productivity; and (2) attempting to study the rate at which new inventions, once made, were diffused throughout the economy, since clearly inventions exert an impact upon resource productivity only to the extent that they are actually adopted in the productive process. The work of Griliches was particularly important in showing that one could explain the diffusion process in considerable detail as a response to economic forces—*i.e.*, on the basis of profit expectations as shaped by market size.[2]

Increasingly, therefore, economists have become more and more confident of their ability to deal with technological events in economic terms. This growing confidence was capped by the publication of a major book by Jacob Schmookler in 1966, called *Invention and Economic Growth* (Cambridge: Harvard University Press). Schmookler argued, quite persuasively, not only that one could explain the *diffusion* of existing invention in economic terms—*à la* Griliches—but that one could even explain the pattern of inventive activity itself.

As a result of these developments, the attitude of the economics profession toward technological change seems to be coming full circle.

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[2] Zvi Griliches, "Hybrid Corn: An Exploration in the Economics of Technological Change," *Econometrica*, October 1957.

Whereas technological change was once regarded as an exogenous phenomenon moving along without any direct influence by economic forces, it is now coming to be regarded as something which can be *entirely* explained by economic forces. Indeed, factors on the technological and scientific levels are increasingly coming to be regarded as not constituting very interesting problems, because we already "know" that we can explain their particular timing in economic terms.[3]

Schmookler's book is obviously very appealing to the economist because it argues that inventive activity is an essentially economic phenomenon, and that it can be adequately understood in terms of the familiar analytical apparatus of the economist. Perphaps I should anticipate my conclusions by saying that I propose to start off from Schmookler's analysis, not because I am in search of a convenient straw man, but rather because I am in substantial agreement with much that he has to say. Moreover, Schmookler's analysis is so rich and so suggestive that it has to be the starting point for all future attempts to deal with the economics of inventive activity and its relationship to economic growth.

II

Schmookler's ultimate interest is, to quote the opening sentence of his book: "What laws govern the growth of man's mastery over nature?" His book represents an attempt to supply building blocks for the answer to that very big question by systematically studying two smaller questions: (1) how to explain the variations in inventive activity in any particular industry over time; and (2) how to explain different rates of inventive activity between industries at a given moment of time. Schmookler's fundamental answer to these questions involves the attempt to link up inventive activity with the structure of human wants and therefore with changes in the composition of demand which are associated with rising *per capita* incomes and other related aspects of economic growth.

The empirical core of Schmookler's book is an attempt to demonstrate, through the study of several American industries, that demand-side considerations are the major determinant of variations in the allocation of inventive effort to specific industries. In examining the railroad industry, for which comprehensive data are available for over a century, Schmookler found a close correspondence between increases in the purchase of railroad equipment and components, and slightly lagged increases in inventive activity as measured by new patents on such items. The lag is highly significant because, Schmookler argues, it indicates that it is variations in the sale of equipment which induce the variations in inventive effort. Schmookler finds similar relationships in building and petroleum refining, although the long-term data on these industries are less satisfactory.

Furthermore, and no less important, in examining cross-sectional data for a large number of industries in the years before and after the Second World War, Schmookler finds a very high correlation between capital goods inventions for an industry and the volume of sales of

[3] The issue is not just whether the scientific and technological spheres are autonomous or not, although that has been a much-debated issue. Even if one were satisfied, for example, that the scientific realm is an autonomous sphere, it need not follow that events in that sphere are unpredictable. They may not be directly influenced by economic variables, but they may be moving subject to an internal logic or an external set of forces which can be identified and then used, by economists, to explain sequences of inventive activity

capital goods to that industry. These data support the view that inventors perceive the growth in the purchase of equipment by an industry as signalling the increased profitability of inventions in that industry, and direct their resources and talents accordingly.[4] Thus, Schmookler concludes that demand considerations, through their influence upon the size of the market for particular classes of inventions, are the decisive determinant of the allocation of inventive effort.

Far from being an exogenous variable as most economists had earlier believed—an activity which, although it had important economic *consequences* was not *controlled* by economic forces—Schmookler concludes that we can treat invention just like any other economic activity. Just as we can analyse production and consumption in terms of revenues and costs and the desire to maximise some relevant magnitude, so we can analyse inventive activity in precisely the same terms.

Schmookler not only attempts to incorporate inventive activity into an economic framework. *Within* that framework he attaches overwhelming importance, as already indicated, to demand forces, and regards supply side considerations as relatively subordinate and passive. Thus, in discussing consumer goods inventions, Schmookler argues that it is the changes in consumer demand over time which are the primary determinant of shifts in the direction of inventive effort.

"* * * (I)f we start out at a given point of time with relative outlays on the different classes of goods given, and allow capital accumulation, technical progress, education, and so on, to occur, then per capita income will gradually rise. In consequence the proportion of income spent on different classes of goods will also gradually change. As different classes of goods become relatively more important than before, the yield to inventive effort in different fields will tend to change correspondingly. And if we further grant that inventive effort is influenced by prospective yield, the direction of inventive activity will shift. Thus, even under the extreme assumption that the *structure* of generic wants is permanently fixed, economic progress will bring successive sections of that structure into play over time, thereby altering the reward structure confronting inventors and rechannelling their efforts accordingly. This is why, for example, American inventors concentrated on food production in the first part of the nineteenth century but gave much more attention in the twentieth century to the requirements of leisure, by creating motion pictures, radio, television, and so on." [5]

Schmookler's argument, as presented so far, would seem to be subject to the fatal objection that its overwhelming emphasis upon demand simply ignores the whole thrust of modern science and the

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[4] Schmookler draws the implication from his data on inter-industry variations in capital goods invention that ". . . *inventive activity with respect to captal goods tends to be distributed among industries about in proportion to the distribution of investment.* To state the matter in other terms, *a 1 per cent increase in investment tends to induce a 1 per cent increase in capital goods invention.*" Schmookler, *op. cit.*, p. 144. Emphasis Schmookler's. It is important to note that Schmookler's results ". . . depend critically on the fact that our capital goods inventions were classified according to the industry that will use them, not according to the industry that will manufacture the new product or the intellectual discipline from which the inventions arise." *Ibid.*, p. 164. See also p. 166.

[5] Schmookler *op. cit.*, pp. 180–1. Of course Schmookler is well aware that consumer expenditure on particular classes of goods is not entirely a function of prices and incomes. Such factors as age structure of a population, climate, geography, and extent of urbanisation, will also play an important role.

manner in which the growth of specialised knowledge has shaped and enlarged man's technological capacities. Such growing technological sophistication, surely, suggests that at least some of the initiative in the changing patterns of inventive activity lies on the supply side and not on the demand side where Schmookler has placed it.

Schmookler has anticipated this objection, and his answer is in fact an ingenious one. He argues that the *commodity classes* towards which inventors direct their efforts are determined by expectations concerning financial payoffs which, in turn, are shaped by the familiar considerations of demand and market size. Developments on the side of science and technology are highly relevant to the inventive process, but only in determining the technical realms—mechanical, electrical, chemical, biological—upon which the inventor will *draw*. While the growth in knowledge at the scientific and technological levels will thus influence the specific *characteristics* of inventions, the *purposes* for which inventions are undertaken will depend upon the state of the market for classes of final commodities.

The point is that, while a marketable improvement in envelope-making equipment is probably about as easy to make as one in glass making, it may be easier today to make an improvement in either field via electronic means than through some mechanical change . . . If differences exist in the richness of the different inventive potentials of the product technologies of different supplying industries, the pressure to improve an industry's production technology tends to be met by the creation of relatively more new products in supplying industries with richer product inventive potentials. For example, if new electrical machines are easier to invent than are non electrical machines, then the aggregate demand for new machinery tends to induce relatively more electrical than non electrical machinery inventions. In brief, inventors tend to select the most efficient means for achieving their ends, and at any given moment, some means are more efficient than others.[6]

Schmookler thus argues for the primacy of demand side considerations, not by suggesting that shifts on the supply side have been unimportant. Quite the contrary. Science and technology have brought about a great transformation in man's capacity to pursue his material ends. But it is precisely because of the *versatility* of man's enlarged inventory of scientific and technical skills that demand side forces retain their primacy.

Oddly enough then, science and technology play a subordinate role in influencing the *direction* of inventive activity within Schmookler's analysis, not because his analysis downgrades their historical significance, but rather because he regards science and technology in the modern age as being, in a significant sense, omnicompetent. Schmookler looks upon the body of modern science and technology as constituting a kind of "putty clay" out of which almost anything can be shaped. As he states, ". . . mankind today possesses, and for some time has possessed a *multi-purpose knowledge base.* We are, and evidently for some time have been, able to extend the technological frontier perceptively at virtually all points." [7]

Now this is precisely the aspect of Schmookler's argument which seems to be most inadequate. If Schmookler is right, then economists

[6] *Ibid.*, pp. 210–11.
[7] *Ibid.*, p. 218. Emphasis Schmookler's.

need not pay too much attention to the internal histories and structures of the sciences and technologies in order to understand the direction of inventive activity. If he is right, then science and technology have not functioned as major independent forces in shaping the timing and the direction of the inventive process. If economic forces can so powerfully shape, not only technology, but science as well, in the achievement of its own ends, then these subjects retain little interest for the economist or economic historian.[8] On the other hand, if Schmookler is wrong in this respect, then his analysis needs to be supplemented by a more careful examination of the manner in which the state of knowledge at any time shapes and structures the possibilities for inventive activity.

III

To establish the independent importance of supply side considerations, it is necessary to demonstrate several things: (1) That science and technology progress, in some measure, along lines determined either by internal logic, degree of complexity or at least in response to forces independent of economic need; (2) that this sequence in turn imposes significant constraints or presents unique opportunities which materially shape the direction and the timing of the inventive process; and (3) that, as a result, the costs of invention differ in different industries.

As soon as one speaks of the "costs of invention" it is necessary to recognize that the economic analysis of inventive activity is seriously handicapped by our present inability to specify the production function for inventive activity with any pretence of precision. Inventions, unfortunately, do not come in units of equal size, whether considered from the point of view of their usefulness or their costs of production. Both the inputs and the outputs in the production of invention are appallingly difficult to measure. Schmookler's basic unit of measurement is, in fact, not an "invention" but a "patent" which serves as a surrogate for an invention. Schmookler's primary interest is in illuminating the process through which society allocates resources to inventive activity. The extreme heterogeneity which is the essence of inventive output is, Schmookler believes, less serious a problem for his interests than it would be in an attempt to link up the number of inventions with the larger phenomena of technological progress and economic growth.[9] Schmookler appears content to regard inventive output

[8] "Thus, independently of the motives of scientists themselves and with due recognition of the fact that anticipated practical uses of scientific discoveries still unmade are often vague, it seems reasonable to suggest—without taking joy in the suggestion—that the demand for science (and, of course, engineering) is and for a long time has been derived largely from the demand for conventional economic goods. Without the expectation, increasingly confirmed by experience, of 'useful' applications, those branches of science and engineering that have grown the most in modern times and have contributed most dramatically to technological change—electricity, electronics, chemistry and nucleonics—would have grown far less than they have. If this view is approximately correct, then even if we choose to regard the demand for new knowledge for its own sake as a non-economic phenomenon, the growth of modern science and engineering is still primarily a part of the economic process." *Ibid.*, p. 177.

[9] See *ibid.*, chapter 2, for a searching examination of the problems involved in using patent statistics as a surrogate for inventions and also for Schmookler's justification for his belief that the deficiencies in the patent data and the problems posed by vast qualitative differences in inventions are less than is generally supposed. For a careful discussion of the measurement problems involved in the economics of inventive activity, see Simon Kuznets, "Inventive Activity: Problems of Definition and Measurement," in R. R. Nelson (ed.), *The Rate and Direction of Inventive Activity*, Princeton, 1962, pp. 19–43.

as adequately measured by the mere number of inventions since, it is important to note, he is not attempting a direct link-up between the inventive process and the larger question of the historical growth in resource productivity. His results, he is careful to point out, ". . . apply only to the number of inventions made, not to their importance. . . . One of the problems of research now is to establish the nature of the connection between the number of inventions in a field and the rate of technological progress."[10] Within this framework the attempt to compare a unit of invention in one industry with a unit of invention in another industry (or even two inventions in the *same* industry) is obviously fraught with difficulty. Schmookler is content to observe that the prospective *value* of inventive output is likely to be greater in industries undertaking large amounts of investment than in industries where such investment is smaller. An industry's volume of investment activity, in other words, is the primary determinant of the profitability of a unit of invention.

This leaves us very much in the dark in attempting to attach a larger significance to a unit of invention. It would be most convenient, for analytical purposes, if there were an identifiable unit of invention which lowered the cost of production in a plant by, say, 1%. This would enable us to assess the importance of a unit of invention by relating it to the size or to the rate of growth of the adopting industry. Unfortunately, the extreme heterogeneity of inventitve output simply does not allow us to assume any simple relationship between the number of inventions and the number of such units of invention or productivity growth.[11] Schmookler does, however, hold the view that the *cost* of invention is likely to be the same in all industries. He points out that ". . . the very high correlations obtained . . . between capital goods invention and investment levels in different industries, and the substantial similarity in the patent-worker ratio of durable and nondurable goods industries indicate that *a million dollars spent on one kind of good is likely to induce about as much invention as the same sum spent on any other good. Hence, doubling the amount spent on one kind of good is likely to induce about as much invention as the same sum spent on any other good.*"[12] This position raises serious difficulties to which we will shortly return.

Although Schmookler's treatment of the relationship between demand forces and invention is, in general, highly illuminating, his conceptual apparatus even here contains some disturbing gaps. This is apparent when by states that, "From a broader point of view, demand induces the inventions that satisfy it."[13] One wishes to rush in at once with qualifications: *some* demand induces the inventions that satisfy it. But which, and when? As soon as these questions are raised we are compelled to consider the different rates at which separate branches of science have progressed. Many important categories of human wants have long gone either unsatisfied or very badly catered for in spite of a well-established demand. It is certainly true that the progress made

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[10] Schmookler, *op. cit.*, p. 163. See also p. 208, footnote 1.

[11] It is, of course tautologically true to say, as Schmookler does, that "A given percentage improvement in productivity is more valuable in a large than in a small industry." *Ibid.*, p. 91.

[12] *Ibid.*, p. 172. Emphasis Schmookler's. See also pp. 209 and 212.

[13] *Ibid.*, p. 184.

in techniques of navigation in the sixteenth and seventeenth centuries owed much to the great demand for such techniques in those centuries, as many authors have pointed out. But it is also true that a great potential demand existed in the same period for improvements in the healing arts generally, but that no such improvements were forthcoming. The essential explanation is that the state of mathematics and astronomy afforded a useful and reliable knowledge base for navigational improvements, whereas medicine at that time had no such base. Progress in medicine had to await the development of the science of bacteriology in the second half of the nineteenth century. Although the field of medicine was one which attracted great interest, considerable sums of money, and large numbers of scientifically-trained people, medical progress was very small until the great breakthroughs of Pasteur and Lister. Improvements in the treatments of infectious diseases absolutely required progress in a highly specific discipline—bacteriology—and the main thrust of medical "inventions" in the past one hundred years would be difficult to conceive without it. Indeed it is highly doubtful that, with the single exception vaccination against small-pox, medical progress was responsible for any significant contribution to the decline in human mortality before the twentieth century.[14]

The point at issue here is one of general importance to Schmookler's argument. The role of demand side forces is of limited explanatory value unless one is capable of defining and identifying them *independently* of the evidence that the demand was satisfied. It would not require a very lively imagination, as the references to medical progress suggest, to compile an extensive list of "high priority" human needs which existed for many centuries, which would have constituted highly profitable commercial activities, but which yet remained unsatisfied. Schmookler's formulation is such that it is capable of being fitted to almost any conceivable set of historical observations. For his argument to be non-tautological, however, it would have to be formulated in such a way that the component element of demand could be identified independently of our observations concerning inventive activity. Until this is done it is difficult to conceive of any set of observations which could directly refute Schmookler's hypothesis. In the absence of a reasonably clear, independent specification of the composition of demand, one can never demonstrate either that important components of demand have gone unsatisfied or that supply side factors played an important role in having down the time pattern of inventive activity.

In fact, the argument of this paper is that, if we want to explain the historical sequence in which different categories of wants have been satisfied *via* the inventive process, we must pay close attention to a special supply side variable: the growing stock of useful knowledge. Historical evidence confirms that inventions are rarely equally possible in all commodity classes. The state of the various sciences simply makes some inventions easier (*i.e.*, cheaper) and others harder (*i.e.*, more costly). In considering the manner in which the stock of scien-

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[14] This is the judgment recently delivered by medical historians. See Thomas McKeown and R. G. Brown, "Medical Evidence Related to English Population Changes in the 18th Century," *Population Studies*, 1955–66, pp. 119–41, and Thomas McKeown and R. G. Record, "Reasons for the Decline of Mortality in England and Wales During the 19th Century," *Population Studies*, 1962, pp. 94–122.

tific knowledge has grown, and the manner in which this growth has, in turn, shaped the possibiliites for inventive activity, one basic fact stands out: The world of nature contains many sub-realms, which vary enormously in their relative complexity. If one considers the broad sweep of scientific progress over the past 300 or 400 years, the timing and sequence of the growth of knowledge in these separate disciplines is closely related to the relative complexity of each—as well as to the complexity of the technology upon which scientific research in the discipline depends. For example, the microbial world and to a great extent the biological world could not be examined without the assistance of the microscope, and the contemporary study of the atomic structure of giant molecules awaited the technique of X-ray crystallography. On the other hand, it is not surprising that the disciplines which were carried to the most advanced state in antiquity were astronomy, mathematics, mechanics and optics. These were each disciplines which could be carried far on the evidence of unassisted human observations, with little or no reliance upon complex instruments or experimental apparatus.[15] Thus, a mastery of the principles underlying the mechanical world was attained long before a similar mastery was achieved over the principles of chemistry—almost 200 years, if we use as our bench-mark dates the publication of Newton's *Principia* on the one hand and Mendelejeff's periodic table on the elements on the other. Similarly, within the discpline of chemistry itself, progress was more rapid in inorganic than in organic chemistry. Even though it had long been apparent that there were huge economic benefits to be reaped throughout the vegetable and animal worlds from a greater knowledge of organic chemistry, such knowledge persistently lagged behind the growing knowledge of inorganic chemistry. Organic chemistry long remained intractable and unresponsive to an obvious and compelling demand. Even after it had become apparent that all organic substances are composed of small numbers of elements—mainly carbon, hydrogen, oxygen and nitrogen—science quite simply remained baflled at the mysteries of the organic world. Progress in organic chemistry, we now know, lagged far behind inorganic chemistry because of a basic and unyielding datum of the natural world: the far greater size and structural complexity of organic molecules.[16] Similar considerations underlie a broad range of research activities and go far towards explaining the timing with which commercial marketable results are extracted from such activities. Thus, the molecular structure of vitamin B_{12}, essential in the treatment of pernicious anaemia, is much more complex than vitamin B_1 or C and, as a result, it took far longer to isolate, synthesise and place in commercial production. Similarly, the comparative lateness of the organic chemist's successful assault upon the structure of protein molecules is largely attributable, we now know, to their great complexity. Amorphous materials, as a

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[15] See T. S. Kuhn, *The Structure of Scientific Revolutions,* Chicago, 1962, chapter VIII and the same author's article, "The History of Science," in the *International Encyclopedia of the Social Sciences.*

[16] The great nineteenth century breakthroughs in organic chemistry in turn laid the basis for the subsequent twentieth century revolution in biology. As Bernal points out: "The new organic chemistry had another essential part to play in the history of science—it was to lead to a fuller understanding of biological processes. In fact, the beginning of any deeper understanding than the microscope could provide was totally impossible without a knowledge of the laws of combination and the types of structure actually to be met with in biological systems. The nineteenth-century development of organic chemistry had to precede logically any attempt to formulate a fundamental biology." J. D. Bernal, *Science in History,* Cambridge, Mass., 1971, 4 vols., vol. 2, p. 633.

group are much more complicated in their atomic structure than crystalline solids and have therefore required a much greater research effort to understand. Progress in the treatment of diabetes has long been held up by the inability to decipher the insulin molecule. Recent research utilising X-ray crystallography has finally revealed a remarkably complex three dimensional structure consisting of no less than 777 atoms. This finding goes a long way towards explaining why a more effective medical programme has taken so much longer to launch in the case of diabetes than in the relatively "simple" diseases such as malaria, syphilis or cholera. Much scientific research at the micro-biological level is, in fact, preoccupied with mapping out the highly complex structural arrangement of the component atoms of organic molecules.[17]

Thus, while I believe that Schmookler has supplied an essential corrective to an earlier, widely-held view which looked upon the scientific enterprise as not only totally exogenous to the economic sphere but even as a completely autonomous force, propelled by a purely internal logic, I also believe that he has overstated his case in some important aspects. Although economic forces and motives have inevitably played a major role in shaping the direction of scientific progress, they have not acted within a vacuum, but within the changing limits and constraints of a body of scientific knowledge growing at uneven rates among its component sub-disciplines. The shifting emphasis of inventive activity over the past two centuries—mechanical, chemical, electrical, biological—is deeply rooted in the history of science, and it is difficult in the extreme to visualise how any plausible set of social and economic forces could have brought about a total reversal of that order.[18] *Given* that sequence in the development of science, inventive activity in some commodity classes was much easier than in others. Furthermore, although Schmookler is doubtless correct that we have an *increasingly* multi-purpose knowledge at our disposal, it is easy to exaggerate the extent to which separate sub-realms of knowledge offer genuine options in the satisfaction of given categories of human wants, in the sense of presenting methods which are *substitutes* for one another. Such substitution is frequently non-existent and usually highly imperfect. Moreover, in many cases the inventive process confronts relationships of complementarity rather than substitution. Thus, the great twentieth century transformation in world agriculture is largely a product of biological knowledge—the mastery of the principles of heredity which have made it possible to develop entirely new, highly productive strains such as hybrid corn in the 1930s and 1940s and, more recently, new wheat and rice varieties. But a fundamental characteristic of these life-science "inventions" is their high degree of com-

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[17] On the great inherent complexity of biological studies Bernal makes the following interesting observations: ". . . [T]he same degree of complexity of even the simplest forms of life is something of an entirely different order from that dealt with by physics or chemistry. What we had admired before in the external aspects of life, in the symmetry and beauty of plants and flowers, or in the form and motion of the higher organisms, now appear, in the light of our wider knowledge, relatively superficial expressions of a far greater internal complexity. That internal complexity is itself a consequence of the long evolutionary history through which living organisms have raised themselves to their present state." *Ibid.*, vol. 3, p. 868. The notion that scientific progress has moved in an orderly sequence from the less complex to the progressively more complex aspects of the physical universe is clearly expressed in Frederick Engels, *The Dialectics of Nature*, Moscow, 1954.

[18] For a brief but highly perceptive treatment of some of the underlying problems, see William N. Parker, "Economic Development in Historical Perspective," *Economic Development and Cultural Change*, October 1961, pp. 1–7.

plementarity with chemical inputs. Indeed, the new high-yielding rice varieties recently introduced into south-east Asia are often no more productive than the traditional varieties if they are grown under the old techniques of crop and soil management. Their unique feature is a high degree of fertiliser-responsiveness brought about by genetic manipulation. A much better name than "miracle" rice would be "fertiliser-responsive." There are no miracles. In fact, the sharp increases in output per acre, which superficially suggest massive improvements in resource *productivity*, are really the result of large increases in fertiliser and other chemical inputs combined with rigorous attention to techniques of water management.[19] Thus, these biological inventions require, for their success, large doses of chemical inputs; fertiliser on the one hand, and pesticides to protect them from the many pests to which they are peculiarly vulnerable, on the other.[20] In this critical area of agricultural technology, then, and in other areas as well, the dominant relationships are those of complementarity and not substitution. In this respect, therefore, our freedom of choice in drawing upon different realms of science and technology for ways of increasing food produtcion is largely illusory. The range within which we can exercise genuine *options* in the achievement of specific goals is, in fact, severely attenuated.

IV

When we move from the realm of science to that of technology, we enter a world where economic motives are much more direct, immediate and pervasive. Since technological concerns are dealt with primarily within a matrix of profit-seeking business firms, one would expect to find, as one does, a high degree of responsiveness to conditions of market demand and profit expectations generally. But here too is abundantly clear that an understanding of demand forces alone provides only very limited insight into the direction and the timing of inventive activity. Here, too, differences in the inherent complexity at the technological level shed a flood of light on the inventive process as it has occurred in historical time. If this is correct, then the Schmookler position that technological problems will be solved (one way or another) when the demand for such a solution is sufficiently pressing (*i.e.*, profitable) is seriously incomplete, and needs to be supplemented by a careful scrutiny of supply side variables.

Consider one of the central events of the industrial revolution: the substitution of a mineral fuel for wood in industrial activities. The growing scarcity of wood and the desirability of substituting coal became increasingly clear in Great Britain as early as the second half of the sixteenth century, during which time the price of firewood rose far more rapidly than prices generally. By 1600 the growing pressure upon the limited supplies of firewood and timber had already produced numerous attempts to introduce coal into individual industries. And yet, in spite of strong and prevasive economic inducements, it took over 200 years before this substitution was reasonably complete. But what is particularly interesting from our present vantage point is that, in *some* industries, the transition to the new fuel was effected

[19] The complexity and costliness of water management methods in the growing of rice is a major reason why the new wheat varieties have so often been introduced more rapidly and with greater success than the new rice varieties. This has been the case, for example, in India.

[20] "We know from experience in the U.S. that the rapid introduction and widespread use of new crop varieties accelerates the biological dynamics of crop disease—host plant relationships."

very rapidly, whereas in others, including some of the most important such as metallurgy, a span of 200 years was required.

Why? A complete answer would be long and complex, but a major part of the answer is that the substitution presented no technical problems at all in some industries, while it created very serious problems in others. No major problems arose in using coal in the evaporation of salt water in salt production, or in lime-making or in brick baking. But in other industries the use of the new fuel seriously reduced the quality of the final product—as in glass-making, the drying of malt for breweries and most importantly, in the smelting of metallic ores. Throughout the seventeenth century considerable effort and experimentation were devoted to these problems. The problems of glass production were solved relatively early by the use of closed crucibles which protected the glass from the destructive effects of the mineral fuel (although, significantly, the method could be used only to produce a coarse cheap glass). In malt production a more palatable beer was being produced by mid-century by first reducing coal to coke and thus eliminating some of the offending elements. Later in the century a reverberatory furnace was introduced which was eventually successfully employed in the smelting of lead, tin and copper. The coke-smelting of iron was first achieved by Abraham Darby in 1709, but the method produced only a very inferior quality of iron. As a result the use of coke pig iron was restricted to the small, cast-iron branch of the iron industry, and charcoal pig iron continued to be used for almost another century for all high quality purposes. It was only after Henry Cort's introduction of the pudding process in the 1780s for the refining of pig iron that the transition to mineral fuel was finally completed.[21]

Thus the timing of a whole series of inventions connected with the introduction of coal can be understood only in terms of a protracted effort at maintaining quality control while introducing coal into industrial uses. The use of coal created a series of new problems, of varying degrees of complexity, in different industries. Moreover, the fuel itself varied considerably in its chemical composition from one region to another. Since the nature of the chemical interchanges between the new fuel and the various raw materials with which it was employed were not understood, a great deal of time was required (in some cases hundreds of years) before crudely empirical methods finally sorted out the economic opportunities presented by the new fuel. Moreover, the sequence in which solutions were found to the problem of different industries varied considerably, depending upon the technical difficulties involved. Indeed, it may be confidently asserted that the solution came *last* in precisely that industry where the economic payoff was greater: the iron industry.[22]

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[21] See T. S. Ashton, *Iron and Steel in the Industrial Revolution*, Manchester 1924; John Nef, "The Progress of Technology and the Growth of Large-Scale Industry in Great Britain, 1540–1640," *Economic History Review*, 1934, pp. 3–24; John Nef, "Coal Mining and Utilization," in Charles Singer *et al.*, *A History of Technology*, London, 1957, 5 vols., vol. 3, pp. 72–88; E. A. Wrigley, "The Supply of Raw Materials in the Industrial Revolution," *Economic History Review*, August 1962, pp. 1–16.

[22] It is interesting to note that the historic links between coal and the iron and steel industry persist even today, in spite of extensive attempts to sever the links. As a matter of fact, one of the reasons for the relatively large size of the coal industry today in the face of strong competition from other fuels has been the inability thus far, in spite of prolonged exploration, to develop a satisfactory technique for producing iron without the use of high-grade coal. Although other fuels have been readily substituted for coal in many uses, the substitution in metallurgical processes poses unique and so far intractable difficulties.

The burden of my argument here is that the allocation of inventive resources has in the past been determined jointly by demand forces which have broadly shaped the shifting payoffs to successful invention, together with supply side forces which have determined both the probability of success within any particular time frame as well as the prospective cost of producing a successful invention. But even if one were to accept the proposition, which I do not, that demand side forces alone determine the allocation of inventive resources, it would still remain true that supply side forces exercise a pervasive influence over the actual *consequences* of such resource use: *i.e.*, the output of successful inventions, and the timing of these inventions. The explanation of the nature and composition of inventive *output* necessarily requires an understanding of the operation of supply side forces. These supply side forces determine whether the output is of the kind associated with the medieval alchemist or the modern scientific metallurgist, the medical quack and patent medicines or broad spectrum antibiotics. Even if knowledge of demand forces alone yielded sensible predictions about the direction of inventive effort, such knowledge, in the absence of further information about supply side forces (the state of scientific knowledge, the prevailing levels of technological skills, the specific characteristics of raw material inputs, etc.) is likely to provide only limited insight into the flow of inventive output.

If we turn to the sequence of invention in textiles, the first major industry to experience full mechanisation, one overriding fact stands out: mechanisation at all stages in the productive process came much earlier to the new cotton branch of the industry than to the older woolen branch. There were several economic reasons for this, which were rooted in the underlying conditions determining the supply of the basic raw materials on the one hand, and the nature of the demand for each of the final products on the other. But, in addition, there was again a fundamental technological fact: cotton production lent itself to mechanisation far more easily than did wool production for reasons intrinsic to the nature of the two materials. As Landes has aptly pointed out:

* * * (C)otton lent itself technologically to mechanisation far more readily than wool. It is a plant fibre, tough and relatively homogeneous in its characteristics, where wool is organic, fickle, and subtly varied in its behaviour. In the early years of rudimentary machines, awkward and jerky in their movements, the resistance of cotton was a decisive advantage. Well into the nineteenth century, long after the techniques of mechanical engineering had much improved, there continued to be a substantial lag between the introduction of innovations into the cotton industry and their adaptation to wool. And even so, there has remained an element of art—of touch—in wool manufacture that the cleverest and most automatic contrivances have not been able to eliminate." [23]

If we consider the sequence in which machine technology was introduced into separate operations in American agriculture, the relative difficulty of applying machine methods to different operations again

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[23] David Landes, *The Unbound Prometheus*, Cambridge, 1969, p. 83. Landes also points out that, even after machinery was introduced into the wool industry, the machines could be operated only much more slowly than in cotton. *Ibid.*, pp. 87–8.

looms up as a critical variable. Why did the reaping and threshing of wheat come so much earlier than mechanisation in cotton picking, corn picking and husking, and milking? Here again, conditions affecting the demand for such individual inventions spring readily to mind. The harvesting of wheat was especially constrained by weather conditions in a way that the other crops were not. The peculiar history of the cotton-growing South provided that region with more abundant labour than other parts of the country and thus considerably weakened the incentive to introduce labour-saving machinery. Yet, as Parker has pointed out, milking operations were also subject to a very strong time constraint and were concentrated in labour-scarce regions of the country where the incentive to invent labour-saving machinery should have been correspondingly strong. Moreover, there is abundant evidence—*e.g.*, from the Patent Office—that considerable, if unsuccessful, inventive effort had been directed toward these operations in the nineteenth century. "Surely the most plausible single answer," Parker suggests, "is that these operations were all inherently difficult to mechanize without radical alteration and improvement of basic elements in the prevailing technology. In the case of the corn harvester, the problem of harvesting the ear separately from the stalk, while preserving the stalk for forage, was hard to solve. In cotton picking, the need to make several passes over the field as the bolls ripened prevented a crude solution. The possibility of mechaical milking was hardly dreamed of, except by cranks, before the gasoline engine and electric power. It is no accident that in all three cases, the mechanical problem was to imitate complex motions of the human hand rather than the simple sweeping actions of the arm required in reaping and threshing." [24]

A large part of the economic history of the past 200 years is, in fact, the story of an enormous outward shift in industrial man's capacity to solve certain kinds of production problems. This growing capacity has been fitful and highly selective. For most of the nineteenth century it involved the exploitation of new power sources and an increasing mastery over the use of large masses of cheap metal (iron and, later, steel). These techniques became available with no fundamental accretions to basic knowledge. They nevertheless were developed slowly because it took time to develop and then to diffuse new techniques in the precision working of metals and to devise the innumerable skill improvements and adaptations which were often required to enable them to operate successfully. There is always a gap, moreover, between the ability to conceptualise a mechanism or technique and the capacity to bring it into effect. Thus, da Vinci's notebooks are full of sketches for novel machinery which could not be realised with the primitive metal-working techniques at his disposal. Breech-loading cannon had been made as early as the sixteenth century, but could not be used until precision in metal working in the nineteenth century made it possible to produce an air-tight breech and properly fitting case. (Without the air-tight breech, a breech-loading cannon was likely to present far greater danger to the persons engaged in firing it than it did to those at whom the fire was being directed.) Christopher Polhem, a Swede, de-

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[24] William N. Parker, "Agriculture," in Lance Davis, *et al.*, *American Economic Growth*, New York, Harper and Row, 1971, p. 385.

vised many techniques for the application of machinery to the quantity production of metal and metal products, but could not successfully implement his conceptions with the power sources and clumsy wooden machinery of the first half of the eighteenth century. Although the principle of compounding was embodied in a patent in 1781, compound steam engines were not introduced into ocean-going vessels until the 1880s, a full century later, in spite of strong economic incentives. Not until major breakthroughs in steel-making technology was it possible to provide high quality components such as boiler plates and boiler tubes upon which the operating efficiency of the compound engine depended. Charles Babbage had conceived of the main features of the modern calculator over a century ago, and had incorporated these features in his "analytical engine," a project which was even favoured with a large subsidy from the British Exchequer. Babbage's failure to complete this ingenious scheme was due to the inability of the technology of his day to deliver the components which were essential to the machine's success.

The purpose of this recitation of frustrations and failures is simply to argue that, given the state of purely scientific knowledge, society's technical competence at any point in time constitutes a basic determinant of the kinds of inventions which can be successfully undertaken. Of course it is possible to argue, as it has been with respect to the long delay in the introduction of a mechanical cotton picker, that if factor prices and/or cotton prices had been significantly different, a practical machine would have been introduced much earlier. If, for example, the available labour supply had been much more expensive, more inventive effort would presumably have been devoted to solving the complex technical problems of a cotton picking machine much sooner. While this is probably true, it is also incomplete. Because it is also true that, *given the set of factor and commodity prices which actually prevailed*, the cotton picking machine would also have been developed more quickly if the technical problems which had to be overcome were less serious. These technical problems and their relative complexity stand independent of demand considerations as an explanation of the timing and direction of inventive activity. Therefore any analytical or empirical study which does not explicitly focus upon both demand and supply side variables is seriously deficient.

Where has this analysis taken us? I have argued that the central weakness of Schmookler's approach is his treatment—or, rather, his neglect—of the supply—responsiveness of technology and invention. Essentially, Schmookler is saying that, given the state of science (and regardless of "how we got here") the supply of inventions is, in effect, perfectly elastic, and at the same price, in all industries. At any moment in time it is possible to get as many inventions as wanted in any industry at a constant price. Therefore the observed *composition* of inventions is entirely a demand side phenomenon, reflecting the manner in which inventive resources have been allocated between industries (or, better, commodity classes) in response to the structure of (demand-induced) profit expectations.

The main objection which I have raised is that inventions are *not* equally possible in all industries. This is because there is a crucial intervening variable: the differential development of the state of sub-

disciplines of science and bodies of useful knowledge generally at any moment in time. Indeed, I think it is very important that we cease talking about "the state of science" and begin thing in terms of "sciences." A central problem is to trace out carefully the manner in which *differences* in the state of development of individual sciences and technologies have influenced the composition of inventive activities. Let me suggest further that one way of getting at this is to pay more attention to historical failures.

Our understanding of inventive activity (and perhaps of social changes generally) is excessively rooted in success stories. We study the history of successful inventions but devote little attention to inventions which were not made. Yet it is highly relevant to ask why it took so long to do certain things, and why inventors failed for so long at some inventive efforts while they succeeded quickly at others. It is certainly possible to study past patterns of research expenditure and inventive efforts, and to seek the reasons for unsuccessful as well as successful outcomes, for very long gestation periods in the development of new inventions as well as for shorter periods.[25] In short, if we want to probe the relations between science, technology and inventive activity more deeply, we must learn much more about what was *not* possible as well as what *was* possible. We need to understand what scientific and technology discoveries were needed for key breakthroughs in invention. For knowledge not only permits—it also constrains. For this reason we can learn much from the study of unsuccessful attempts to invent something for which the market was perceived to be ready. In this respect, the study of failure is essential to a determination of the precise role of supply side variables in the inventive process. After all, the demand for higher levels of food consumption, greater life expectancy, the elimination of infectious disease, and the reduction of pain and discomfort, have presumably existed indefinitely in the past, but they have been abundantly satisfied only in comparatively recent times. It seems reasonable to suppose that the explanation is to be found in terms of supply side considerations. It is unlikely that any amount of money devoted to inventive activity in 1800 could have produced modern, wire-spectrum antibiotics, any more than vast sums of money at that time could have produced a satellite capable of orbiting the moon. The supply of certain classes of inventions is, at some times, completely inelastic—zero output at all levels of prices. Admittedly, extreme cases readily suggest arguments of a *reductio and absurdum* sort. On the other hand, the purely demand-oriented approach virtually assumes the problem away. The interesting economic situations surely lie in that vast intermediate region of possibilities where supply elasticities are greater than zero but less than infinity!

The perspective which I am suggesting, therefore, states that, as scientific knowledge grows, the cost of successfully undertaking any given, science-based invention declines—from infinitely high, in the case of an invention which is totally unattainable within the present state of knowledge, down to progressively lower and lower levels.

[25] It is worth mentioning here that our lack of interest in the study of failures may also have contributed in an important way to an under-estimation of the costs of invention. In our preoccupation with success stories we inevitably ignore the substantial commitment of resources to unsuccessful inventive efforts, and recognise only those which were connected with a successful outcome.

Perfectly inelastic supply curves of invention gradually unbend and flatten out. (To what *extent* they flatten out is, of course, an impirical question, on which Schmookler has adopted the arbitrary and implausible extreme assumption of perfect elasticity.) Thus, the growth of scientific knowledge means a gradual reduction in the cost of specific categories of science-based inventions. The timing of inventions therefore needs to be understood in terms of such shifting supply curves which gradually reduce the cost of achieving certain classes of invention. More precisely, we need to think in terms of a number of supply curves for individual industries, depending upon the knowledge bases upon which inventive activity in that industry can draw, and we need to understand more clearly the extent to which different "pools" of knowledge are potential substitutes in the inventive process. Schmookler's hypothesis states, in effect, that there is one supply curve for all industries and that the extent of substitution renders it unnecessary to look at supply conditions in individual industries. It seems to me that a clear articulation of the relations between science, invention and economic growth requires a critical examination of this assertion. The basic economic question, of course, is not an "either or" proposition telling us whether a particular technological achievement is or is not possible at a particular point in time. The economic question is: Given the state of sciences, *at what cost* can a technological end be attained? How does the state of individual sciences differentially structure the cost of society's technological options?[26] Answers to these questions will carry us a long way towards a deeper understanding of both the nature of inventive activity and the process of economic growth by providing further insight into the economy's changing capacity to respond to economic needs.

Market Structure and Innovation: A Survey

By Morton I. Kamien and Nancy L. Schwartz

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I. INTRODUCTION

Economics, we all recite, deals with allocation of limited resources towards satisfaction of unlimited wants. Resources are typically identified as land, labor, and capital plus a technology that determines their transformation into consumer goods. Disparity between the available goods and services and the desired gives rise to scarcity and the question of what, how, and for whom to produce. The focus then shifts to description and evaluation of alternative resource allocation mechanisms for making the choices. The Pareto criterion, by which an allocation of resources is deemed efficient if any reallocation improving the position of some individual worsens the position of others, is a commonly employed gauge of a mechanism's performance. In the absence of externalities, increasing returns to scale, and uncertainty, a perfectly competitive market system yields a Pareto optimal allocation of resources; this underlies the view that individual self-interest

[26] Note that my emphasis upon supply and cost considerations does not imply any sort of scientific or technological determinism. More costly inventions can always precede less costly ones in time if demand conditions are sufficiently strong.

is compatible with society's interest. The further conclusion that Pareto optimality may not be achieved via the market system in the presence of monopoly elements provides an economic rationale for antitrust laws.

The objective of a resource allocation mechanism appears to be, according to the analysis described above, to make the best of available resources. The alternative objective of relaxing constraints through expanding the resource base or developing new technology seems to be beyond its scope. Thus, until rather recently, technical advance had been regarded, in the mainstream of economic theory, as unmotivated by the quest for profits and substantially unaffected by resource allocation. Instead, as J. Schmookler observed, technology had been viewed as a parameter like the weather, affecting the outcome of resource allocations but itself unaffected by them [84, 1965]. Evidence that technological progress has significantly contributed to growth in productivity, together with a substantial increase in research and development activity, largely financed by government and carried out by industry (see F. Machlup [51, 1962]), may have spurred reconsideration of this view.

Once technical advance is regarded as an economic variable, it is natural to inquire if the perfectly competitive market continues to retain its efficiency in this extended scope and if not, what arrangement would be superior. Alternatively, the question is whether we can continue to rely on individual self-interest to provide an efficient aggregate outcome. Unfortunately, the question is not fully resolved on the theoretical level. Further, empirical studies have not provided compelling support for any side of the controversy. Since antitrust patent and copyright law, and government financing may influence the course and rate of technical advance, determining how they do so is of interest to policy makers as well as to theorists.

Few, if any, economists maintain that perfect competition efficiently allocates resources for technical advance. The dispute is between those who contend that an imperfectly competitive market system is the best alternative and those who deny it, maintaining instead that collective action in the form of some degree of government financing of research and development is superior. Among those contending that imperfect competition is best, there is a wide range of opinion regarding the optimal degree of imperfection; *i.e.*, the optimal firm size and/or degree of rivalry.

Many issues regarding determinants of technical advance are intimately related to other facets of economic inquiry such as public goods, imperfect competition, uncertainty and search, decentralization, and growth, which themselves are incompletely understood. We shall not attempt to bring recent advances in those areas to bear on the questions addressed here; nor shall we consider the question of factor bias in technical change, recently surveyed by V. K. Smith [88, 1973].

A few words on terminology are in order here. Realizing a technical advance involves a sequence of activities, typically beginning with recognition of a physical, biological, or social principle and terminating with a commercial product or service. The earliest stages, commonly called basic research, are characterized by uncertainty regarding the discovery of a true principle and by possible disregard

for its eventual usefulness. In the next stage, known principles are used to create prototypes of new products or processes. This invention or research component of R&D is characterized by focus on a specific goal and uncertainty regarding its technical feasibility and economic cost. The final stages, development and commercialization (marketing) of a new or improved item, motivated by profit, are marked especially by uncertainty about the extent of the market and planned introduction dates of rivals. In actuality, a technical advance seldom follows the orderly sequence described. Instead, there may be overlapping through feedback among the stages as described by E. Ames [3, 1961] and V. W. Ruttan [77, 1959], and flowcharted by Machlup [51, 1962], who also describes current practice in classification. We shall not distinguish between invention and innovation or basic research and R&D here, except to the extent that an article reviewed exclusively addresses one of these facets of the process. Likewise, we shall not in general distinguish between process innovations, that reduce the cost of producing an existing item, and product innovations. The distinction rests in part on who uses the innovation. A new or improved product of one industry may be a process innovation for the industry supplied. We shall, however, attempt to distinguish between the process by which resource allocation affects technical advance and the effects of market structure and firm size on the allocation of resources to this activity.

In this review, selected knowledge about the inventive process is considered first, emphasizing the measurement of its inputs and outputs and the role of "technological opportunity." The results of innovative activity, particularly their impact on market structure, are noted next. Arguments about which institutional arrangements are most conducive to innovative activity begin with the Schumpeterian defenses of monopoly power and bigness. A wide range of empirical interpretations of the Schumpeterian hypothesis has led to a diversity of tests.

Hypotheses frequently tested involve relationships between R&D activity and firm size, each variously measured. Less frequently recognized are the interrelationships among these hypotheses. Logically first in the effect of firm size on the technical innovation production function, *i.e.*, on the transformation of reserach inputs (effort) into research output (inventions; innovations). Studies have indicated that many development projects would typically be more efficiently done in a relatively small firm than in a large one (see *Section* II. C. below). Put another way, these studies indicate that by and large there are no economies of scale with respect to firm size in the invention process.

The fact, however, that many R&D projects could be done more efficiently by small firms than large ones does not tell us anything about where the bulk of R&D is done or about relative R&D effort or the achievements of firms of varying sizes. A question reviewed in *Section* V. A. is whether larger firms spend more on R&D relative to their size than smaller firms, *i.e.*, whether the intensity of innovational effort increases with firm size. As will be seen, the bulk of the evidence indicates that, among firms engaged in R&D, relative effort tends to increase with size up to a point and then decline, with middle

size firms devoting the most effort relative to their size. (Those studies focus primarily on firms that do have sustrained formal R&D programs. A separate but related question is that of participation in R&D; here the evidence is complementary, indicating that while there are important interindustry differences in participation rates, most small firms do not engage in research and most very large firms do.)

The question of the relation between firm size and research output intensity is taken up in *Section* V. B. Conceptually this is the product of the two preceding questions; research effort of a size-group, multiplied by its efficiency in doing research yields its research output intensity. Roughly speaking, combination of research efficiency that decreases with firm size beyond a quite moderate size, and research input intensity that increases and then decreases beyond some point leads to anticipation that research output, relative to firm size, should increase with size but then decline with further increases in firm size. Within most industries, we would expect that the peak research output intensity would occur at a smaller firm size than the peak research input intensity, due to decreasing efficiency. In fact the evidence indicates that research output intensity does tend to increase and then decrease with increasing firm size. However, to our knowledge, there has been no comparison made of the firm sizes at which maximum research input and research output intensities occur.

Another major effort related to the Schumpeterian hypothesis has been determining the influence of market structure on resources devoted to inventive activity and on inventive output. Again efficiency of the inventive process plays an intermediary role between inputs and outputs. Measures of market structure commonly employed in these studies indicate at best something about the intensity of rivalry among the firms in a particularly defined industry. A major omission of the studies is the consequence of potential rivalry, so much stressed by Schumpeter, on inventive activity. Thus, empirical definition and measurement of potential rivalry, and evaluation based thereon, remains a key element for our understanding of this phenomenon. Efforts in this direction at the theoretical level are reviewed in the last section. Finally, there are what might be called indirect tests of the Schumpeterian hypothesis, involving association between supposed advantages of size or power and innovative activity; these are discussed in *Section* VII.

Our review is highly focused, selective and not exhaustive. For excellent comprehensive reviews of what is known about R&D in its many facets, see, in addition to sources already mentioned: E. Mansfield [54, 1968]; R. R. Nelson, M. J. Peck, and E. D. Kalachek [63, 1967]; F. M. Scherer [82, 1970]; or J. M. Blair [8, 1972]. K. Norris and J. Vaizey [64, 1973] is very recent and provides a nice overview. Other valuable reviews include P. Hennipman [33, 1954] (on the classical debates), R. E. Johnston [38, 1966] and L. Weiss [97, 1971] (both on the evidence about technical progress and market structure), and J. M. Vernon [94, 1972].

Comparative case studies appear in C. F. Carter and B. R. Williams [12, 1957], J. Jewkes, D. Sawers, and R. Stillerman [36, 1969], S. Myers and D. G. Marquis [59, 1969], J. M. Langrish, *et al.* [47, 1972], and C. Layton [48, 1972] among others. K. Pavitt and S. Wald

[67, 1971] have reviewed and synthesized a vast amount of empirical evidence on the factors influencing technological innovation: a substantial portion of their references are to sources other than the academic economics journals.

II. THE INVENTIVE PROCESS

A. *Research inputs and inventive outputs*

As noted above, including the state of technology among the variables of an economic system is relatively recent, still requiring some justification. We begin therefore, with a look at some evidence of a positive relationship between inventive activity and the level of resources devoted to it.

To go from the abstract concept of inputs into the innovation process to its concrete empirical embodiment poses difficulties. Usual input measures include R&D spending, R&D employees, and scientists and engineers employed. Each of these, as well as other indices of innovational effort, has recognized deficiencies. For instance, technical improvements may not only be developed in the R&D department but also in operating and other divisions. Using measures of scientific personnel accommodates this phenomenon, but also includes employees with no R&D function or contribution. Modified indices of R&D employment often include a distinction between total R&D personnel and professional R&D personnel. "Technical personnel" may be refined into all technical personnel, or all scientists, or scientists and engineers.

Other measures are susceptible to institutional distortions. David Novick noted two significant jumps in reported R&D spending without commensurate increases in R&D activity [8, Blair, 1972, pp. 201–204]. First, a 1954 change in the tax treatment of research expenditures provided an incentive for firms to classify additional activities as "research." Second, Sputnik and its aftermath made R&D more fashionable, particularly in military and space related areas. The "fashion" aspect extends to other industries, where R&D may be viewed favorably by stockholders.

Innovational output has been measured alternatively by patents awarded, important patents awarded, important inventions or innovations, and sales of new products. Deficiencies of patents as an output measure include the facts that the patent recipient need not have been responsible for the invention, that patented inventions are of unequal importance, and that some important inventions are not patented. Nevertheless, systematic study of patenting behavior has led Schmookler, Scherer and others to conclude that the number of patents granted a firm is a usable proxy for inventive outputs.

Schmookler found in his intensive study of patents and patenting that nonpatenting is not a serious problem before 1940, but post-1945 corporate patenting failed to keep pace with invention [85, 1966]. This was attributed to both the lengthening period required to obtain patent approval and a more hostile political and legal attitude towards patents, especially in connection with exclusive licensing. Consequently, post-1945 and pre-1940 patent statistics are not comparable. B. Branch took into account this shift in patenting behavior in his

study of the relationship between patents awarded firms and their real sales growth; he ran separate regressions for the pre- and post-war periods [10, 1973]. The regression coefficient of the patent variable was about one-fourth as large for 1928–39 as for 1950–64.

Some studies employed only "important" patents or inventions, as evaluated by judges with competence in the field. Schmookler found that time series of important inventions in a number of fields resemble those of all inventions in the field. A final measure of inventive output, employed by W. S. Comanor, is sales volume of new products in years immediately following their introduction [14, 1965]. While sales volume reflects the significance of the invention, it also depends on the size of the market and other factors.

Schmookler found similarity between changes during the period 1870–1950 in the numbers of scientists and engineers and numbers of patents [85, 1966]. He also found patents and R&D expenditures to be closely related in 1953 for 18 major industry groups, with 85 percent of the interindustry variation in patents pending explained by the variation in R&D expenditures. Based on a sample from the 500 largest U.S. industrial firms in 1955 Scherer's regression analysis indicated a very nearly linear relationship between the number of R&D personnel in 1955 and the number of patents issued a firm in 1959 (the four-year lag reflecting) the average time for filing and processing patent applications) [79, 1965]. In another study, Comanor and Scherer compared three measures of innovative activity for 57 pharmaceutical firms, namely sales volume of new products (inventive output) within two years after introduction, number of R&D personnel, and number of patents received [16, 1969]. With firm size held fixed, the correlation between patents and other measures of R&D input and output was positive and statistically significant.

Mansfield found, for given firm size, a close relation in the long run between the rate of R&D spending and total number of important inventions forthcoming [53, 1968]. Comanor's study of the pharmaceutical industry reached a similar conclusion [14, 1965]. Pavitt and Wald found across 13 U.S. industries a high correlation between R&D intensity (R&D funds/sales, 1964) and rates of technical innovation measured by the expected annual rate of introduction of new products as a percent of sales [67, 1971]. The also found across ten Organization for Economic Co-operation and Development (OECD) countries, high correlation between national industrial R&D expenditures and national performance in technological innovation, after correcting for population differences. There seems little doubt that, on average, a direct relation between innovational effort and innovational output exists. However, it is likewise true that the transformation may depend on factors other than effort, and it may not be linear.

B. Technological opportunity

Acknowledging a positive relationship between inventive output and prior devotion of resources to inventive activity raises a further question. Does the presence of basic knowledge, also called "technological opportunity," stimulate inventive activity or is the stimulus the profit potential of innovations that satisfy an existing want? Denial of the latter possibility would preclude explaining inventive activity as an economic phenomenon. It would also deny any influence of market

structure on invention. The evidence, however, appears to support both possibilities.

The role of basic science is reflected in interindustry differences in both R&D effort and innovational output. A Phillips is a leading spokesman for the positive role of "technological opportunity" *i.e.*, the extent of basic scientific knowledge in the field [70, 1966; 71, 1971]. He argued that extensive R&D programs and high innovation rates occur only over a period of time where there is related but exogenous scientific progress. If progress in the science slows or moves in directions leading to fewer opportunities, technical progress in the market is slowed. A very similar argument is advanced independently by N. Rosenberg [75, 1974].

To qualify and distinguish differences in technological opportunity. Scherer ran cross-sectional linear regressions by industry of patents granted on sales [79, 1965]. Interindustry differences accounted for about as much of the total variance in corporate patenting as did interfirm differences in sales volume. The bulk of interindustry variation (that was unrelated to sales) was attributed to differences in the underlying science base, or technological opportunity. The 14 industries were grouped into four broad classes, based on the regression coefficient of patents awarded, that accounted for most of the interindustry differences in patenting relative to sales. The groupings were (1) electrical, (2) general chemicals and stone, clay, glass, (3) moderates (petroleum, rubber products, fabricated metal products, machinery, transportation equipment), and (4) unprogressives (food and tobacco, textiles and apparel, paper and allied products, miscellaneous products, miscellaneous chemicals, primary metals). L. Philip's regression of research workers on total workers for 12 Belgian industry groups likewise revealed interindustry differences accounting for about as much of total variance as interfirm differences [72, 1971]. Moreover, the aggregation, based on regression coefficients, of the 12 industries into four groups led to no significant loss in explanatory power relative to the 12 groups. Although Scherer used innovational output (patents) and U.S. data, while Philips used research input (R&D personnel) and Belgian data, the classification of industries was strikingly similar. Further evidence of the role of technological opportunity is provided by T. M. Kelly's investigation of 181 large multiple-product firms within six industry groups [44, 1970]. Measuring 1950 inventive effort by the ratio of R&D employees to total employees, he found technological opportunities played a positive, significant role in the innovative activity of the chemical and petroleum industries relative to other industries studied.

Comanor associated "technological opportunity" with ease of achieving product differentiation [15, 1967]. Based on McGraw-Hill surveys of the purposes of R&D and his own studies of the pharmaceutical industry, he speculated that a major goal of R&D is development of new, differentiated products that afford a protected market position [13, 1964; 14, 1965]. He then hypothesized that research effort would be greater in industries where the prospects for successful product differentiation are better. Comanor identified consumer durables and investment goods as industries with high potential for products differentiation and consumer nondurables and material inputs as those with

little. Research effort was adjusted for firm size by using "predicted" research for the average firm size within each size class. The adjusted research levels were grouped by adjudged possibility of product differentiation. Comanor found, consistent with his hypothesis, that research levels tended to be far greater in industries producing consumer durables and investment goods than in those manufacturing consumer nondurables and material inputs. The distinction in research by product type was especially pronounced among large firms (over 25,000 employees).

Schmookler, on the contrary, found "technological opportunity" provided little stimulus [85, 1966]. Chronologies of hundreds of economically or technologically important inventions in four fields indicated the stimulus was typically a technical problem or opportunity conceived largely in economic terms. In contrast to many accounts which cited economic problems as the immediate stimulus, a scientific discovery was not specified as initiating invention in any instance. A comparison of time series of important inventions and of patents within four fields lent no support to the hypothesis that inventions in a field beget further invention.

Schmookler argued that invention is largely an economic activity, pursued for gain; that expected gain varies with expected sales of goods embodying the invention; and that expectations of sales for improved capital goods are largely based on present capital goods sales. The number of capital goods inventions would therefore be expected to vary over time and among industries directly with and in response to current capital goods sales. Time series behavior of capital goods inventions was compared with that of investment for railroading, building, and petroleum refining, and similarity was found. Cross section regression analyses suggested that, on average, increasing the investment of an industry tends to increase—in about the same proportion—the number of capital goods inventions.

In Schmookler's study, inventions were attributed to the industry expected to use them; in contrast, other investigators classified inventions by the industry expected to supply them. To see if this difference affected the conclusions, Schmookler regressed, in logarithmic form, patents granted in 1959 on alternate measures of 1955 industry size using Scherer's data classified by industry supplying the invention. A similar regression employed his own attribution of inventions by usage. The coefficient of size was significant and close to unity in each case. However, the latter regression yielded a far higher correlation coefficient. He inferred from these results that, on average, increasing the market served by an industry tends to increase the number of goods invented for it to produce in about the same proportion, just as increasing investment in an industry tends to increase proportionately the number of capital goods inventions for it to use. However, the appreciably smaller portion of variance explained when inventions are classified by source suggested that an economic objective can be achieved by a variety of technological means, with the most efficient usually selected. The rise of the chemical and electrical industries may be a consequence of inventors' problems being relatively efficiently solved by chemical and/or electrical means, Schmookler said.

In the discussion thus far, technological opportunity has been implicity viewed as unlimited though unequally available. The hypoth-

esis that technical possibilities become depleted was also tested by Schmookler. The time series of patents for railroad track was similar to that of patents for all other railroad patents. The resemblance in patterns of invention, despite differences in underlying technology, suggested that technical progress slowed as it became less valuable, not because it approached exhaustion. The volume of invention in different aspects of shoe manufacturing was likewise similar over time. Contrary results were recently reported by M. N. Baily investigating new drug introduction [5, 1972]. He found the number of new drugs introduced in any year was positively related to R&D spending in the pharmaceutical industry in preceding years (the development period) and negatively related to a seven-year moving average of past total (all sources) new drug introductions.

C. Technology of the innovative process

If resource allocation influences technical advance, what is the nature of the relationship? Is the innovation process essentially a lottery so that devotion of more resources improves the chance of winning, or is it governed by deterministic rules? In-depth case studies of the innovation process, reviewed below, shed some light on this question. How is resource allocation related to the quality of innovations, to their quantity, and to their speed of development? Are these economies or diseconomies of scale in any of these dimensions? Alternatively, is innovation facilitated by concentration of ever large numbers of scientists engineers, and technical staff along with equipment, or is there a size beyond which no further advantages obtain? The technology of the innovation process may influence market structure in much the same way as the technology of production does, defining minimum scale, optimal scale, number of firms in an industry, and ease of entry. J. H. G. Olivera, for example, demonstrates that the presence of unlimited economies of scale in the innovation process would dictate its concentration in a single unit for efficiency [66, 1973]. The empirical studies reported below relate to these questions, beginning with time-cost trade-off in development, proceeding to the effect of scale on the efficiency and quality of innovation, and ending with the determinants of successful innovation.

According to Scherer the cost of development increases more than proportionately with contraction of the development period for several reasons, *i.e.*, the time-cost trade-off is convex to the origin [80, 1967]. First, since R&D involves learning through time, compression of the development period curtails the learning process. Second, the uncertainties associated with R&D necessitate experimentation. Sequential experimentation takes more time but results in fewer false starts and fruitless duplications. Compressing development time requires more costly parallel experimentation. Third, time compression requires more personnel employed per unit time leading to classical diminishing returns. Mansfield, *et al.* in particular, found support for this relationship by estimating the time-cost trade-off functions for 29 completed innovations by 11 firms in the chemical, machinery, and electronics industries, using interview data [55, 1971].

Conceptually there are two major "scales" that may affect efficiency and quality of innovation. First is the effect of firm size on the efficiency of a given size R&D facility. Second is the effect of scale of the

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R&D facility for a given size firm. These two questions are conceptually distinct and their answers can have quite different policy implications. For instance, economies of scale related to firm size would indicate that concentration of sales could enhance R&D. On the other hand, economies of scale related to the size of the R&D facility would support cooperative R&D or concentration of public R&D funds in fewer but not necessarily larger firms. There is more evidence on the first question than the second, perhaps due to the relative availability of data and sometimes tacit assumption of multicollinearity. Empirical studies over the last 10 years have typically shown that while there may be certain advantages of size in exploiting the fruits of R&D, it is more efficiently done in small to medium size firms than in large ones. The extent to which efficiency varies with the size of the R&D program itself, for given firm size, has been little investigated. These two issues are reviewed together below.

Comanor [14, 1965] attempted to discern, within a sample of 57 pharmaceutical firms, the relationship between research input, average R&D employment 1955–60, and new product output (the share of new chemical entities or of new products in firm sales). No evidence was found that the presence of a large support staff relative to professional staff substantially increases efficiency of a pharmaceutical research facility. Rapid growth of the entire R&D facility did not seem to impair research efficiency. Marginal productivity of professional research personnel appeared inversely related to firm size. Estimated elasticities of research output with respect to research input suggested economies of scale in R&D at low firm sizes but diseconomies as a firm becomes moderately large. A. S. Angilley reported constant or modestly increasing returns to scale in the innovation production function for an international sample of 20 pharmaceutical firms, based on regressions of innovative output (new product sales 1958–70 or new products weighted by therapeutic significance) on 1969 R&D expenditure [4, 1973].

Schmookler, reviewing the broad evidence, found that beyond a modest level, efficiency of inventive activity tends to vary inversely with firm size [86, 1972]. Large firms spent more on R&D per patent pending in 1953 than did smaller ones. The larger cost per patent is not attributable to a differential propensity to patent by firm size: Schmookler cited independent findings indicating that small firms employ a greater proportion of their patents commercially than large firms. Mansfield found for 10, 8, and 11 major firms in the chemical, petroleum, and steel industries respectively, that the number of significant inventions per dollar of R&D spending was lower in the largest firms than in the small and medium size firms [53, 1968]. His study also indicated that if firm size is held constant, increases in R&D expenditures results in more than proportional increases in inventive output in the chemical industry. No such advantage of increasing R&D effort was evident in the other two industries, however. Mansfield, *et al.* found a ranking of R&D programs of major chemical firms and of 9 petroleum firms on the basis of overall quality and effectiveness per research dollar expended varied directly with the firm's total R&D budget and inversely with its sales [55, 1971]. W. N. Leonard found, for 16 two and three digit industry groups, that while firm-financed

research intensity was positively associated with subsequent growth, federally supported R&D was not [49, 1971]. High federally financed R&D in the aircraft and electrical equipment industries resulted in low marginal productivity of such expenditures. Scherer, through regressions found that patent intensity (patents per billion dollars of sales) varied inversely with firm size and increased with R&D intensity (R&D employment per sales dollar) but showed diminishing returns [79, 1965].

A. C. Cooper interviewed about 25 development managers with experience in both large and small companies or in rapidly expanding development organizations in either the electronics or chemical industry [17, 1964]. He also obtained actual cost figures for a particular parallel development effort by a large company and a small company. Remarkably consistent estimates indicated a given product would cost three to ten times as much to develop by a large firm as by a small one. Larger firms, he found, seem to become enmeshed in bureaucracy and red tape, resulting in a less hospitable atmosphere for creative contributions by operating personnel. Superior technical personnel tend to be attracted to smaller companies where greater latitude may be afforded them. The larger the firm, the more difficult it may be to recognize the problems needing solution. Finally there is evidence of greater cost consciousness in smaller firms.

Not only is invention and development more costly to large firms, but some firms have also suppressed them. Blair finds evidence of this in the synthetic rubber, automatic glass machinery, shoe machinery, cable, braking systems, matches, and golf clubs industries, among others [8, 1972]. Sluggishness of large firms in developing certain innovations has been attributed by Blair to a desire to protect an investment in current technology, satisfaction with the status quo, underestimation of potential demand for a new item, neglect of the inventor, and misdirection of research as well as incompatibility of bureaucracy and creativity.

Regarding quality or importance of inventive output, D. Hamberg's review of his own and others' findings led him to conclude that large industrial labs tend to produce mainly minor inventions [31, 1966]. He claims that the fraction of total inventive output of these labs identifiable as "important" is less than the comparable ratio for inventive output of other sources. Mansfield, *et al.* found modified support for Hamberg's thesis in their study of 1964 R&D programs of 13 major chemical and 8 major petroleum firms [55, 1971]. Up to some point, larger firms tended to devote a larger fraction of R&D expenditures to basic research, to have more technically progressive projects, and to have longer expected completion time. However, there was little difference in these areas between behavior of the largest firms and that of firms half as large.

L. L. Duetsch has dissented, at least regarding the ethical drug industry [18, 1973]. He queried physicians and developed a list of important new drugs appearing during 1940–67. The contribution of ethical drug manufacturers to the discovery of important drugs (90 percent of U.S. total) was comparable to their participation in discovery of all new drugs (87 percent of U.S. total). Jewkes, Sawers, and Stillerman found the sources of invention diverse and the large research

labs of industrial corporations not responsible for the bulk of significant inventions [36, 1969]. Pavitt and Wald examined empirical evidence developed during the 1960's and concluded that both large and small firms play essential, complementary, and interdependent roles in the process of innovation [67, 1971]. Larger firms have tended to contribute most to innovation in areas requiring large scale R&D, production, or marketing. Smaller firms tend to concentrate on specialized but sophisticated components and equipment. They have often made very major innovations when large firms let the opportunity slip by.

Finally, emphasis on good management throughout the firm, with excellent working relationships and communications among the R&D, production, and marketing departments is a recurring theme in comparative studies of success and failure in innovations. Mansfied, *et al.* insist these interrelationships cannot be overemphasized [55, 1971]. Besides these factors, Carter and Williams also emphasize the role of communications between supplier firms and customer firms in stimulating and being receptive to technical advances [12, 1957]. C. Freeman studied 58 attempted, paired innovations (29 failures and 29 successes) in chemicals and in scientific instruments [24, 1973]. Failure and success, as well as similarity in pairs, were defined in terms of market; not technical characteristics. Most attempts to innovate involved a formal R&D structure and patenting, although no systematic differences in R&D organization or incentives could be discerned. The distinguishing characteristics were related to marketing, broadly interpreted. Successful innovation generally involved greater attention to education of users, to publicity, to market forecasting, and to selling. Most significantly, successful innovation was marked by an understanding of user needs throughout all functional areas of the firm, including the R&D and production departments as well as marketing.

In case studies of innovations in 10 industries by Layton and others, British companies were compared with companies in the United States and continental Europe [48, 1972]. Particular attention was given the role of qualified scientists and engineers at every stage from idea to successful innovation. Many instances were found in which skillful initial invention in the R&D department did not lead to successful innovation because of failure to carry through with skillful production planning and/or marketing. Good communication between the development and marketing departments was found essential in the capital goods industries. Langrish and others studied 84 innovations that won Queen's Awards in the United Kingdom in 1966 and 1967 [47, 1972]. Myers and Marquis reviewed 567 innovations in five U.S. industries [59, 1969]. Both studies found that in initiating innovation, the clear identification of a need that could be met was important more often than the realization of the potential usefulness of the discovery.

In sum, there appears to be an inverse, convex time-cost trade-off in the innovations process. There generally appear to be economies of scale in the innovation production function up to a modest size, with further scale economies in transformation of R&D effort into innovational output (quantity and quality) being exceptional. In-depth studies have begun to isolate some firm and managerial characteristics associated with success in innovation.

III. RESULTS OF INNOVATIVE ACTIVITY

Innovation can lead to greater profits and growth for the innovator and can affect industry market structure. Minimum scale for economic production may either rise or fall as a result. The need to finance an R&D program in order to become a viable industry member may impose a barrier to entry. Innovation may provide a means of entering an industry or of increasing an existing firm's market power. Finally, research and the diversification of firms into similar industries may be related.

The impact of innovative activity on subsequent economic growth and improved productivity has been extensively studied and recently reviewed; see M. I. Nadiri [60, 1970]. C. Kennedy and A. D. Thirlwall [45, 1972], and Z. Griliches [30, 1973]. At a 1972 colloquium it was concluded that, despite methodological difficulties and debates, all the evidence—at the level of the firm, industry, and economy—indicates that the contribution of R&D to economic growth/productivity is positive, significant, and high [61, 1972]. Estimates of rates of return range between 10 percent and 50 percent with a bias toward the higher values. A positive association between sustained R&D programs and/or innovations and subsequent growth and profitability at the firm level has been noted by Mansfield [53, 1968], Smith and Creamer [89, 1968], and Baily [5, 1972] among others.

Blair very comprehensively reviewed the literature dealing with the impact of technological advance upon economies of scale [8, 1972]. He concluded that from the late 18th century through the first third of the 20th century, technical change exerted a powerful impetus toward concentration because advances in steam power, in materials and methods of fabrication, and in transportation (railroad) permitted an encouraged scale expansion. Since then, newer technologies (electricity, materials and methods of fabrication, trucks) tended to have the opposite effect, reducing plants size and capital requirements for optimal efficiency. Hamberg reached similar conclusions [32, 1967]. The most recent technical advances have not only tended to permit economic production with smaller plants, but have also increased effective rivalry to older products by widening the range of substitutes.

R&D and technical progress can be barriers to entry, particularly in certain industries with high "technological opportunity." Phillips has argued that R&D and innovative behavior by existing firms tend to define, along with other factors, limits to entry by new firms [70, 1966: 71, 1971]. The lower the product price and the greater the exploitation of opportunities presented by exogeneous scientific and technical developments, the less likely is entry of additional firms. He found support for this hypothesis in an extensive study of the commercial aircraft industry. Effective entry into this industry has been achieved only through major technical advances affording substantial cost and performance advantages for carriers. Relatively low operating costs seem to have been a necessary, but not sufficient, condition for a plane to capture a sustained larger market share. The effect of technical advances as a barrier to entry in the commercial aircraft industry has been offset somewhat by an apparent proclivity by successful manufacturers to remain too long with their original success. These firms have not always

continued to be scientifically progressive and thereby retain their market position.

Other industry studies reached similar conclusions. Comanor found R&D is a major element of interfirm rivalry in the pharmaceutical industry, with profit largely dependent on a firm's continued innovative success [13, 1964]. Effective entry usually requires an innovation, so that cost and risk of research, as well as high selling expenditures, constitute an entry barrier to this industry. Freeman found R&D to be an entry barrier in the oligopolistic international electronic capital goods industry [21, 1965]. Rivalry occurs mainly in technical innovation and technical customer service. Because of the products' complexity, their manufacture employs many existing patents, through cross-licensing, know-how agreements, and patent pools. The stronger a firm's own technical position, the more readily it can obtain such technical agreements on favorable terms. At a minimum, a firm must have a strong development and engineering capability to assimilate, imitate, use, and improve upon inventions of others. This minimum R&D capacity to maintain a defensive market position plus requisite marketing and technical service facilities constitutes a minimum size for entry.

D. C. Mueller and J. E. Tilton embedded the notion of R&D costs as a barrier to entry within a stages-of-development of the industry hypothesis [58, 1969]. Their review of available evidence plus two case studies (semiconductors, photocopying) of their own indicate that during the industry's infancy neither relative nor absolute size are requisite to invention, development, or technical imitation. In the next stage, with many firms in the industry, the basic science well understood, and the research relatively sophisticated and specialized, substantial costs of building and maintaining a large R&D capability constitute an entry barrier favoring the large industrial lab. The final stage, maturity, according to Mueller and Tilton, is one in which basic patents expire and production techniques are standardized. Barriers to entry are not based on R&D requirements but on production and marketing scale. Price competition replaces technological competition.

Pavitt and Wald found opportunities for small firms tend to be greatest in the earliest stages of the "product cycle," when economies of scale are relatively unimportant, market shares volatile, and rates of entry and failure high [67, 1971]. Successful entry is largely dependent on scientific and technological capability at this stage. As technologies mature, scale and efficiency in production become more important and opportunities for small firms fewer.

We have discussed the role of R&D in inhibiting entry by imposing a minimum scale. Phillips assigns R&D a still larger part in fostering oligopoly; he argues that initial successes in a technologically changing industry create opportunities for further successes while failure may breed further failure. Because of substantial learning costs, the successful firm is immediately placed in a superior position, while other firms fall into an inferior position; further progress becomes easier for the former and increasingly difficult for the latter. As a result, the scale of the successful surviving firms tends to be large and industry concentration to be high. Phillips sought, without much success to support his theory with regression analysis of 11 U.S. industry groups [70, 1966]. He observed that monopoly power can be achieved in several ways and that oligopolistic firms need not have

been technologically progressive. He then undertook a detailed investigation of the U.S. commercial aircraft market during 1932–65. Advances in science and technology relating to aircraft manufacture often arose outside the industry but provided opportunities and incentives for manufacturers to develop new commercial aircraft. He concluded that the stream of innovations had the hypothesized effects. The number of manufacturers decreased with large shifts in market shares of the remaining [71, Phillips, 1971].

The hypothesis of "success breeds success" also is supported in H. G. Grabowski's study of R&D in the chemical, drug, and petroleum industries [28, 1968]. One of three major determinants of firm research intensity was found to be an index of the firm's prior research productivity, measured by the number of patents received per scientist and engineer employed. In the sample, firms with higher patented output per scientific worker in the past were, *ceteris paribus*, more research intensive than their rivals. If Grabowski's findings are broadly indicative, past R&D success tends to lead to greater current R&D effort, which in turn could be expected to produce further innovational output and, in general, a widening gap between the technologically successful firms and their rivals.

Finally, there is evidence of a positive statistical relationship between research activity and diversification. Diversification is the extent to which firms classified in one industry produce goods classified in another. Patterns of interindustry diversification seems to be the same whether it proceeds by internal development or acquisition. M. Gort in his study covering 1929–1954, found that industries entered most often by diversifying firms and industries in which diversifying firms were most frequently based were industries characterized by high technical personnel ratios [27, 1962]. He also noted a positive relation between industries entered and productivity increases. A. Wood examined diversification over 1959–1962 and found that industries of frequent entry or origin were characterized by high proportions of R&D expenditures to sales, controlling for growth, profitability, and other variables [99, 1971]. He suggests that high research intensity tends to encourage and facilitate diversification into similar industries.

IV. MONOPOLY POWER AND LARGE SIZE ARE GOOD

Foremost among those associated with the position that monopoly power and a large size spur inventive activity is Schumpeter [87, 1950]. He envisioned an economy as an organism with cells constantly dying and being replaced by superior ones. By this process of "creative destruction," the organism grows and flourishes. In an economic system, regeneration and growth are achieved through replacement of existing products, processes, and modes of industrial organization by improved ones. Schumpeter saw the quest for extraordinary profits through innovation as the motivational force propelling the process of creative destruction. By the very nature of this process, the monopoly position achieved through innovation is temporary. It is susceptible to erosion by imitation and usurpation by new innovations.

Innovation, however, requires a relatively sizable commitment of resources and a commensurate return to make it worthwhile. Immediate imitation of a firm's new product or process by others, as in perfect competition, would eliminate realizable rewards and thereby its incentive to innovate. Thus, only a firm that can attain at least temporary monopoly power, delaying rival imitation will find innovation attractive. Faced with choice between perfect competition, with its static efficiency properties and little incentive for innovation, and imperfect competition with innovation, Schumpeter opted for the latter. Indeed, he maintained that is the constant fear that a favorable position won through innovation would be lost to others via the same route that is the paramount relevant form of competition. To quote,

"* * * in capitalist reality, as distinguished from its textbook picture, it is not that kind of competition (price) which counts but the competition from the new commodity, the new technology, the new source of supply . . . It is hardly necessary to point out that competition of the kind we now have in mind acts not only when in being but also when it is an ever present threat. The business man feels himself to be in a competitive situation even if he is alone in this field." [87, Schumpeter, 1950, pp. 84–85].

H. H. Villard refers to this situation as "competitive oligopoly" [95, 1958]. Phillips observes that continual innovation may be viewed as the analog of limit pricing to retard entry in the traditional form of price competition [71, 1971]. To bolster his argument, Schumpeter maintained that a resource allocation mechanism's performance should be gauged through time rather than at an instant of time.

The promise or possession of monopoly profits alone, however, is not enough to bring forth vigorous innovative activity. J. K. Galbraith emphasizes the importance of firm size *per se*. [25, 1952]. He contends that the era of cheap innovation is past; diminishing returns have set in. Current innovative activity requires vast sums of money for technical personnel, engineers, scientists, and their equipment. The needed resources are available only to large firms possessing a substantial degree of monopoly power. According to G. W. Nutter, "just as the prospect of monopolistic position raises the odds in favor of the most risky innovations, so bigness makes possible the most expensive" [65, 1956, p. 524]. Moreover, large firms can hedge against the technical uncertainties associated with innovation by undertaking several projects simultaneously. The diversified firm may also, according to Nelson, be more likely to find use, and have argued that large size and monopoly power are prerequisites for economic growth through technical progress. The promise of monopoly power for a time creates the quest for it through innovation, while fear of its loss promotes continued innovation and adoption of new technology. The loss to society from the absence of perfect price competition is more than compensated by the gains in the long run, derived from innovation. The admitted conflict between individual and society interests in the presence of inducible technical change can, according to Schumpeter, be reconciled by a departure from perfect competition. Regarding policy, Schumpeter argued that antitrust law should be implemented more selectively so as not to impede technical progress.

V. EVIDENCE ON FIRM SIZE

The Schumpeterian hypothesis is broad and has been even more broadly interpreted. Empirical tests have covered a range of components of the hypothesis. A statistical relationship between firm size and innovative activity is most frequently sought, with exploration of the impact of firm size on both the amount of innovational effort and innovational success (output), (as distinct from the impact of firm size on the efficiency of innovative effort, discussed in *Section* II. C. above). A relation between monopoly power, measured by industry concentration, and inventive activity has likewise been sought. Finally, there have been indirect tests of the hypothesis, seeking statistical association between inventive activity and various supposed advantages of size and market power. F. M. Fisher and P. Temin have recently tried to reduce an interpretation of the Schumpeterian hypothesis to a quantitative, testable form and to show how various alleged tests are related [20, 1973]. They emphasize that these tests are by no means identical, with one neither broader range of uses, for the uncertain outcome of an R&D project than will a single product firm [62, 1959]. He implies that a single-product firm is unable to exploit an invention not directly linked to its primary product, through licensing to others or developing a new product line.

Thus Galbraith and D. Lilienthal [50, 1953] argue that industries composed of several large firms, each with a measure of monopoly power, will engage in more innovative activity than those consisting of many small firms with little market power. To the challenge that fewness of firms inhibits price competition and also stifles innovative competition, Galbraith replies that innovation plans of rivals are more difficult to detect than pricing decisions (a questionable assertion in light of the work by G. J. Stigler and J. K. Kindahl [93, 1970] and are therefore not susceptible to the same pressures. O. Lange [46, 1943] observes that the desire to avoid price competition in an industry will lead to innovations that do not increase industry output, a view shared by Y. Brozen [11, 1951].

Reduced price competition (due to oligogpoly) need not affect small firms and large firms in the industry in the same way. Phillips reasons that if price competition is tacitly inhibited, non-price competition among the largest firms in the industry is likewise curtailed [69, 1965]. It is primarily a tool of small firms seeking profit improvement by introducing new subsititutes for the existing product. A moderately concentrated industry may have firms large enough to realize the advantages of size in innovation, while market power is diffuse enough that rival retaliation is not an overriding consideration. Phillips conjectures that there might exist a degree of rivalry intermediate between prefect competition and monopoly that is most stimulating for innovative activity.

In brief. Schumpeter and his followers J. S. Worley found this elasticity significantly greater than unity in only 2 of the 8 industries represented by 198 very large firms [100, 1961]. Interestingly, Worley noted a tendency for firms near the middle of the size distribution to hire relatively more R&D personnel than the largest and smallest firms of the sample.

Comanor fit log-linear regressions with 1955 and 1960 data for 387 firms in 21 groups [15, 1967]. The estimated elasticity of research employment with respect to firm size (employment) was never significantly greater than unity and was significantly less than one for 7 of the 21 industries. Comanor suggested that his coefficients may have been smaller than Hamberg's because of the latter's broader industry classes; apparently, larger elasticities may reflect aggregation of heterogenous industries.

Scherer criticized the data and the form of the regression employed in these studies [78, 1965]. At best, he contended, the results reveal the relationship between research intensity and firm size for firms with sizable research programs, since firms without large research efforts were generally omitted from the samples. He also noted that the log-linear form of the regression equation cannot reveal inflection points or nonmontonicity in the relationship between input intensity and firm size. To overcome these weaknesses, Scherer, using 448 of the 500 largest firms, regressed 1955 R&D employment against the first three powers of 1955 sales and against powers of the logarithm of sales. The cubic equation involves high collinearity of the independent variables, but permits detection of inflection points and nonmonotonicity in the relationship. Scherer found the relationship between R&D employment and firm size typically had an inflection point, with R&D employment increasing faster than firm size among the smaller firms in the sample but more slowly among larger firms. R&D employment may even fall with increasing size among the very largest firms in some industries. Finally, Scherer noted that the chemical industry and the giant leaders of the auto and steel industries may be exceptional; their R&D intensity appeared to rise with sales.

Mansfield estimated a log-linear relation between R&D spending over 1954–1959 and firm size for 10 major firms in the chemical industry, 9 in petroleum, 8 in drugs, 7 in steel, and 4 in glass [53, 1968]. The coefficient of firm size did not shift systematically over time. Except for chemicals, the largest firms in these industries spent no more on R&D relative to sales than did somewhat smaller firms. Grabowski regressed research expenditure, 1959–1962, against sales and its square for 16 major chemical firms and 10 major drug companies [28, 1968]. Among the drug firms, research intensity initially increased but then decreased over most of the relevant range of firm size. In contrast, research intensity increased steadily with firm size for the chemical industry. Further examination of the data led Grabowski to suggest that the difference in the observed relation between research intensity and firm size in the two industries was largely attributable to factors other than size that are significant in explaining research intensity.

A. Firm size and inventive effort

The first question to be reviewed here is whether larger firms spend more on R&D relative to their size than smaller firms. To examine the question, a measure of innovational activity is deflated by a measure of firm size to obtain an index of innovational intensity, *i.e.*, innovational effort or inventive output relative to firm size.

Firm size is alternately measured by sales volumes, assets, or number of employees. These three variables are positively but not perfectly correlated, and the results depend somewhat on the variable selected.

Scherer discusses their features and relative merits [78, 1965]. He prefers "sales" because it is neutral regarding factor proportions and R&D budgets may be based on projected sales. The deflator is typically comparable with the numerator when possible. Thus R&D spending is often deflated by sales or assets, R&D employees and scientific personnel by total employees.

Early studies suggested at most a very weak positive association between R&D input intensity and firm size. With 1947 and 1951–1952 data, I. Horowitz found industry ranking by value added per establishment to be positively, but weakly correlated with both the breadth of participation in research by firms and research expenditures per sales dollar [34, 1962]. For 340 of *Fortune's* 500 largest firms in 1960, grouped in 17 industries, Hamberg found the ratio of R&D employment to total employment to be only weakly correlated with total employment and total assets [31, 1966]. Log-linear regression revealed that the elasticity of R&D effort with respect to firm size exceeded unity in only 3 of the industries.

Smith and Creamer analyzed National Science Foundation (NSF) data on industrial R&D during 1957–1965, classified by industry and firm size (a medium-size firm has 1000–4999 employees) [89, 1968]. R&D input intensity, measured by company financed R&D per net sales dollar, averaged 1.4, 1.5, and 2.1 cents for the small, medium and large firms respectively. Small firm R&D input intensity was no less than that of medium size firms in six of the twelve industries and exceeded that of large companies in three industries. Similar conclusions obtained when R&D input intensity was measured alternatively by R&D scientists and engineers per 1000 employees.

D.C. Muller's four equation econometric model of the firm, fit using a sample of 67 firms over 1957–60, indicated that research intensity was negatively associated with firm size measured by sales [57, 1967]. Kelley's multiple regression of the ratio of R&D employees against the logarithm of total assets and weighted market share failed to reveal a relationship between R&D intensity and either variable in his sample of 181 firms [44, 1970].

In a cross-sectional study of 301 Belgian firms, Philips found that regressions of research personnel upon powers of total employment yielded a cubic as the statistically best fitting equation [72, 1971]. It indicated that the number of research workers in Belgian firms grows faster than total employment up to about 7000 employees, and at a decreasing rate thereafter. Research intensity (research employees per 1000 total employees) peaked at about 10,000 employees. This relationship for the aggregative economy was not replicated in individual industries, there being great diversity in industry patterns. In most industries, however, the elasticity of research employment was less than one.

W. S. Adams compared research activity in the United States and France to test the influence of firm size [1, 1970]. He argued that since U.S. firms are larger than French ones, if absolute firm size were conducive to R&D, then R&D spending would be more highly concentrated among the very largest firms in France than among the largest U.S. Firms. The share of R&D performed by the largest 300 firms in each country that was attributable to the 4, 8, 20, etc. largest

firms was cumulated and compared. Total R&D performance (private plus government funds) was less concentrated in France than in the United States, suggesting that absolute firm size is not conducive to R&D activity. Adams' regression analysis also revealed that R&D intensity in France is unrelated to firm size.

Thus, it seems that with the possible exception of the chemical industry, there is hardly any support for the hypothesis that the intensity of innovational effort increases with firm size. Reviewing previous literature, J. W. Markham concluded that innovational effort tends to increase more than proportionately with firm size up to some point that varies from industry to industry [56, 1965]. For still larger firms, innovational intensity appears to be constant or decreasing with size. Subsequent investigations are consistent with and tend to reinforce that generalization.

Three related caveats are worth noting. First, there are interindustry differences in the relation between size and innovational effort. Second, much of the evidence on the effect of size has not controlled for other factors that may be helpful in explaining innovational effort. Size may prove to be either more or less important as an explanatory variable once these other factors are discovered and taken into account. Third, the evidence on innovational intensity relates to firms that do have a sustained R&D effort, and does not reflect research participation rates. The vast majority of large firms have, and the vast majority of small firms do not have, sustained R&D programs.

B. Firms' size and output

Innovational output has also been used as a measure when studying the influence of firm size on innovational intensity. Mansfield had trade personnel list and rank by importance the major innovations during 1919–1938 and 1939–1950 in three industries [53, 1968]. The largest four firms in the coal and petroleum industries were found to be responsible for a larger share of their respective industry's innovations than of its productive capacity, but the largest four steel producers were responsible for fewer. In a later study, he found the market share of the four largest pharmaceutical firms exceeded their share of the industry's innovations [55, 1971]. In this industry, the large firms' performance was slightly better in terms of weighted than unweighted innovations, where medical weights were based on judgments of physicians and pharmacologists and economic weights were based on sales during the first 5 years following introduction.

Mansfield used these data to estimate the firm size at which innovational intensity is greatest. While the regressions fit the data only moderately well, the results varied considerably among industries. Maximum innovational output intensity occurred at about the size of the sixth largest firm in the petroleum and coal industries. In the steel industry, however, very small firms exhibited maximum intensity. For pharmaceutical advances over 1935–1949, the maximum corresponded to the size of the tenth largest firm. Peak intensity of weighted pharmaceutical innovations over 1950–1962 was found at about the twelfth largest firm; for unweighted innovations, it occurred at the size of very small firms.

Scherer used the number of patents issued a firm in 1959 as proxy for average inventive output 4 years earlier [79, 1965]. Within a sample of 352 firms from the 1955 *Fortune* 500, sales volume was consistently more concentrated among the largest firms that R&D employment, which in turn tended to be slightly more concentrated than patents. Smaller firms in the sample were responsible for a higher relative share of inventive activity than sales. A regression of patents on the first three powers of sales, fit for 448 firms, yielded a relationship increasing at a decreasing rate up to a point of inflection of $5.5 billion sales. Only three firms in the sample were larger than this. The regression was repeated for the 14 industries and also for 4 consolidated industry groups. The essential findings were unchanged; patent output generally increased less than proportionately with sales among large corporations.

D. J. Smyth, J. M. Samuels, and J. Tzoanos made a similar study of 86 United Kingdom firms in chemicals, electrical engineering and electronics, and machine tools [90, 1972]. Large firms were found more likely than small ones to participate in patenting. Patents granted during 1963–1966 were regressed on the first two powers of firms size (1963 net assets), profits, and cash flow. The number of patents awarded increased more than proportionately within the chemical industry and, for all but the largest firms, within the electrical engineering and electronics industry. In contrast, patenting decreased with firms size in the machine tool industry.

Freeman reported on a recent study of some 1200 post-1945 innovations in Britain [23, 1971], that small firms (under 200 employees) accounted for about 10 percent of industrial innovations since 1945, compared with about 25 percent of employment and 21 percent of net output. The rank ordering of industries by the share of small firms in industry innovations corresponded fairly well with the ordering by share of small firms in net output. Small firms contributed more than their proportionate share of innovations in industries characterized by low entry costs and low capital intensity and development costs for many products; they contributed little, either absolutely or relatively, to innovations in industries of high capital intensity.

B. Johannisson and C. Lindstrom studied 181 Swedish firms with over 500 employees, in 12 industrial sectors [37, 1971]. Excepting the four largest firms, large firms' share of total patent applications in 1965–66 (inventive output) was consistently less than their share of employees in 1966 (firm size). Patent applications increased faster than firm size among members of the chemical industry, but less than proportionately with firm size in engineering and metal manufacturers.

Thus the conclusion about the effect of size on innovational effort tends to be supported and reflected in evidence on size and innovational output. Beyond some magnitude, size does not appear especially conducive to either innovational effort or output in either this country or in European countries where studies have been conducted. However, patterns differ by industry. It seems noteworthy that the chemical industry is cited as an exception both for the United States and abroad. The reason for this exception appears to be in the technology of chemical process plants; Freeman found that new plants con-

structed around 1968–70 tended to be some four to five times larger than new plants built around 1950–52 [22, 1968]. In case histories of 31 process innovations in the industry, he discovered that two-thirds cost over a million dollars each, while many cost over $5 million.

VI. MARKET STRUCTURE AND INNOVATION

W. R. Maclaurin studied the effect of market structure on innovation, comparing a ranking of 13 U.S. industries by their important innovations during 1925–1950 with their ranking by the extent of monopolization in 1950 [52, 1954]. Each ranking was judgmental, corroborated in the first instance by number of patents issued, number of scientists with doctorates, and presence or absence of a research department with responsibility for developing new products and processes. The ranking by monopolization considered size of industry price leaders and ease of entry. The two rankings of industries did not coincide. He concluded that while some degree of monopoly power is necessary for technological progress, it is not sufficient. Ease of entry. entrepreneurial leadership, and a "competitive spirit" were considered nearly essential. Maclaurin also emphasized the role of the underlying engineering art or scientific base.

Maclaurin's early conclusions have stood the test of time and subsequent investigation rather well. Some factors he emphasized have been quantified and their roles subjected to statistical analyses. The index of monopolization that is most readily available is a "concentration ratio," measuring the portion of industry sales attributable to the 4 (8, 20) largest firms. The limitations of this measure have been amply discussed elsewhere; see, for example, Blair [8. 1972] and Scherer [82, 1970]. In particular, concentration reflects the current sellers of a product and may be quite unrelated to the extent of actual and potential rivalry in innovating new products; see *Section* IV above and *Section* VIII below.

A. *Concentration and research effect*

The hypothesis that research input intensity is positively associated with concentration of industry sales has been tested with varying results. Horowitz found the four-firm concentration to be positively but weakly associated with research expenditure per industry sales dollar and negatively correlated with percent of industry research labs in the largest 20 percent of the firms [34, 1962]. Hamberg also found a weak positive correlation between company-financed R&D per sales dollar and industrial concentration [31, 1966]. These findings weakly suggest that more highly concentrated industries tend to put forth a greater research effort. Further. participation in research seems more widespread in more concentrated industries. so the greater research intensity is not merely the reflection of the largest firms' efforts.

In Scherer's re-examination of the hypothesis. research effort. the dependent variable. was measured by various indices of technical employment [80, 1967]. Independent variables included a concentration index, 1960 industry employment, and such qualitative factors as the technological opportunity class and type of good (producer or consumer, durable or nondurable). Scherer's first test involved logarithmic regressions. Coefficient of the concentration variable were positive

and highly significant when qualitative factors were omitted. When these factors were introduced through dummy variables, the cofficients of the concentration index remained positive and statistically significant, but the incremental explanatory power of "concentration" was far smaller.

In the second test, research effort intensity, measured by technical employment as a fraction of total employment, was regressed on the concentration index and qualitative dummy variables. The concentration coefficient showed modest significance. In a third test, with regressions of technical employment intensity against concentration for the two technology classes with the most observations, the concentration coefficients were positive and significant. Scherer concluded that the hypothesis of positive association between concentration and the intensity of research effort was supported. The fact that the incremental explanatory power of concentration fell sharply on introduction of dummy variables was attributed to the positive correlation between concentration and technology class. Scherer suggested this last correlation might support Phillips' hypothesis that technological innovation arising from opportunity has led to increased concentration.

Scherer also tested the hypothesis that increases in concentration are conducive to technical vigor only in relatively atomistic industries, becoming unimportant once a certain threshold is crossed. The square of the concentration index was added in the third set of regressions indicated above. The relation between technical employment intensity and seller concentration was found to be concave, but the coefficient of the squared term was significant in only one case. In all four cases, technological employment per 1000 employees reached a predicted maximum at concentration levels between 50 and 55 percent. The threshold level appeared to be above 10–14 percent.

In Comanor's study, estimated elasticities of research effort were regressed against average firm size and an eight-firm concentration ratio [15, 1967]. The coefficient of average firm size was reported positive and significant, but no effect of concentration was apparent. Comanor conjectured that since product differentiability may be an important component of the research decision, the effect of concentration upon research might depend on whether differentiability is high. To test this, industries were grouped by whether the eight-firm concentration ratio exceeded 70 percent. Research levels, adjusted for firm size by size class, were grouped by both concentration and differentiability classes. Comanor concluded that high concentration tended to be associated with much research in cases where it is not a major element of market behavior; that is, where prospects for product differentiation are relatively weak.

Most recently, Kelly's study disclosed that maximum research intensity appears to occur at a 50 percent to 60 percent concentration ratio, in close agreement with Scherer's results [44, 1970]. Likewise, the four-firm concentration ratio and its square have significant coefficients when dummy variables reflecting technological opportunity are omitted, but are not significant otherwise.

Adams tested the hypothesis of positive association between seller concentration and research activity by comparing R&D spending in-

tensity and the four-firm concentration index by industry in France and in the United States [1, 1970]. He found that for high-technology industries except instruments, the country with the larger concentration index had the smaller R&D spending intensity. Among lower technology industries, results of the comparison were mixed. The hypothesis was rejected. Regression analysis of French data indicated that differences in research intensity among the French firms were unrelated to differences in seller concentration.

Phillips tested the hypothesis using Belgian data [72, 1971]. The number of research personnel was regressed against total personnel and the concentration index. Provision was made for each technological opportunity group to have its own coefficient for the concentration variable. Slope coefficients were positive for both the chemical and electrical equipment industries but not significantly different from zero for the moderate and nonprogressive groups. As a corroborating test, Phillips regressed research intensity (research personnel as a fraction of total employees) of the four largest firms on the concentration ratio, permitting each technological group to have its own slope coefficient. Concention had a significant influence only in the chemical and possibly in the electrical equipment industries. Phillips concluded that for Belgian industry, concentration and research effort tend to be positively associated in those industries with greatest technological opportunity, that is, in those industries where research is most intensive.

S. Globerman studied the roles of concentration and technological opportunity on research effort in Canadian manufacturing industries, 1965–69 [26, 1973]. The 15 industries were divided into 9 with greater technological opportunity and 6 with lesser, based largely on Scherer's classification. R&D personnel per 1,000 employees, among firms engaged in R&D, was regressed against a four-firm concentration index, the fraction of industry assets held by non-Canadian corporations, and a measure of governmental subsidy. Globerman found that for industries with greater technological opportunity, research intensity varied inversely with concentration (and directly with both foreign ownership and government financing). All coefficients were highly significant. In contrast, for industries with lesser technological opportunity, all signs were reversed but no significant relationship was present.

In reviewing the diverse findings on research efforts and concentration, we find little consensus. In most instances, it has been difficult to discern a statistical relationship between these variables. There is agreement that the relation may vary with the "technological opportunity class" of the industry. Comanor found for the United States a positive association where technological opportunity is weak, while Phlips, using Belgian data, found the positive association where the technological opportunity is great. Adams, in comparing the United States and France, found a negative association where technological opportunity is great. Globerman's conclusions for Canada were similar to those of Adams. While the diversity of findings could be explained on the grounds that different countries were studied, it seems more prudent to conclude that the relation between research effort and concentration warrants more study on a disaggregated basis. Concentra-

tion is unlikely to be a good proxy for the extent of active rivalry in an industry.

B. Concentration and innovative output

Studies relating concentration to productivity increases have found high concentration alternatively harmful, neutral, and helpful. In each case, the degree has been moderate. Stigler [92, 1956] compared the rate of technical progress, measured by the decline in unit labor requirements, 1899–1937, in 14 industries of high concentration with 7 industries in which concentration was declining and 8 in which it was low. The largest reduction in labor requirements was in industries in which concentration fell substantially during the period and the smallest in industries of continued high concentration. These statistical results reinforced Stigler's assessment of the broad facts, suggesting that competition of new rivals in an industry spurs rapid technical advance. B. T. Allen [2, 1969], updating Stigler's study using data for 19 industries over various periods during 1939–1964, found no significant differences in productivity growth rates by industry concentration class.

Using changes in productivity of labor and horsepower per employee as indices of technical change, Phillips found that in 28 U.S. industries over 1899–1939, industries with high concentration or large factories showed greater technical change [68, 1956]. Carter and Williams [12, 1957] followed the pattern of Phillips' study for 12 United Kingdom industries for 1907–1948; there was some positive correlation between the degree of concentration and the increase of output per employee-hour. Weiss found productivity growth in the United States positively related to output growth in both 1937–1948 and 1948–1953, but no significant association was found between average four-firm concentration and productivity increase in either period [96, 1963]. B. Bock and J. Farkas did find a positive association between productivity and concentration in the United States for 1963 [9, 1969].

Scherer tested the hypothesis that technological output tends to increase with industrial concentration [79, 1965]. The number of industry-related patents issued in 1954 to the leading four firms in the industry was regressed against their sales and the four-firm concentration ratio as well as dummy variables for technological class. No support for the hypothesis was found.

To explain his findings about the bituminous coal, petroleum refining, and steel firms (*Section* V. B. above), Mansfield developed a model that predicts the largest four firms in an industry will tend to account for a relatively large share of the innovating in cases in which (a) the investment required to innovate is large relative to the size of potential users, (b) the minimum size of firm required to use the innovation profitably is relatively large, and (c) the average size of the largest four firms is much greater than the average size of all potential users of the innovations [53, 1968]. This model explained Mansfield's data in the three industries mentioned and in the railroad industry as well. Apparently, it was not applied to his 1971 pharmaceutical data. It is far more elaborate than a concentration ratio and goes deeper into structural aspects of the industry to explain relative innovational contributions.

O. E. Williamson [98, 1965] found a simple hypothesis was also consistent with the data developd by Mansfield. He regressed the largest four firms' share of innovations, relative to their share of the market, against the concentration index both linear and log-linear forms of the equation. The influence of concentration on the relative innovational performance of the largest four firms was found to be negative. The relative share of innovations contributed by the largest firms appeared to decrease with their monopoly power. For a concentration ratio above 30–50 percent, the largest firms appear to supply less than their proportionate share of innovations.

The inconclusiveness of studies on concentration and innovational effort is reinforced by the studies discussed in this section. Mansfield's work suggests that even "technological opportunity class" may not be enough to sort out the underlying relationship sought; a deeper study of components of industrial structure may be required.

C. Other elements of market structure

The ease of entry into an industry is an element of market structure that might influence research intensity. Comanor has argued that a principal goal of research activity is creation of entry barriers through product differentiation [15, 1967]. Therefore, he thought research outlays would tend to be low where high entry barriers of other forms were present. To test this hypothesis, average research personnel, adjusted form firm size, was regressed against dummy explanatory variables, reflecting classification of an industry by its concentration, by its opportunities for product differentiation, and as having a high moderate, or low entry barrier due to scale economy. Comanor found no significant effect of the high entry barrier or of low entry barrier. However, moderate entry barriers appeared to have a positive and significant impact, after other factors were taken into account. Comanor rejected his originial hypothesis and revised his views. He concluded that when entry barriers are either quite low or very high, the incentive for research may be substantially less than at some intermediate level. Industrial research effort appears strongest in industries with some entry barrier, causing rapid imitation to be impeded, but it is also strongest where entry itself has not been effectively foreclosed.

H. O. Stekler found support for the hypothesis that the U.S. aerospace industry has become increasingly technologically progressive as the Federal government, its major customer, has become decreasingly protective of industry members [91, 1967]. Johannisson and Lindstrom studying Swedish industry found no effect of a firm's market share on its patent applications when firm size is taken into account [37, 1971]. In Freeman's paired comparison study of success and failure in industrial innovation, the "competitive environment" of the would-be innovator did not seem to influence success of the attempt to innovate [24, 1973]. Freeman emphasized that the presence of competitive pressures may nevertheless be important in stimulating attempts to innovate.

The stimulating effect of rivalry on R&D activity was recently investigated by H. G. Grabowski and N. D. Baxter [29, 1973]. Using a sample of eight chemical firms during 1947–1966, they tested the hypothesis that firm R&D expenditures respond positively to a rival's

R&D outlays. A multiple regression involving current changes in the firm's R&D expenditure on lagged changes in its and a rival's R&D outlays, changes in its cash flow and market value, and a dummy variable to reflect sales or earnings decline was fitted. The relevant rival was selected by best statistical fit as either the firm with the largest R&D outlay or the immediate successor in magnitude of R&D budget. Cash flow appeared to be the single most important explanatory variable. Change in rival's expenditure on R&D was significant in four cases and the firm's own lagged spending change in three. Neither firm valuation nor the dummy variable offered considerable explanatory power. Thus this phase of the study provided some evidence of responsiveness among firms to each others' inventive activity, especially between the two leading firms. Grabowski and Baxter, seeking further confirmation, then tested the hypothesis that rivalry in R&D will be stronger the more oligopolistic the industry. They argued that as concentration increases, the firm's R&D intensities will become more similar, as reflected by a decreasing coefficient of variation in firm research intensity. Using a sample of 29 three-digit industries and an eight-firm concentration ratio, they found a significant negative relationship in the rank correlation between concentration and the coefficient of variation of research intensity. Thus concentration does appear to induce conformity in R&D expenditure among firms. It should be noted, however, that this does not imply that high concentration leads to high levels of research activity. An industry in which no research takes place also exhibits complete conformity among its members along this dimension.

Our review of the impact of market structure on innovation has netted little more than reaffirmation of the early observation that both competitive pressures and market opportunity seem important. Further work is required. There are two suggestions in the literature. First, evidence on both size and the market structure elements indicates the sought after relationships are quite likely nonlinear. Intermediate values of the market structure elements may be most conducive to research effort and its success, with extreme values providing less incentive. Second "technological opportunity," broadly interpreted, seems to condition the relationship. It is not clear whether the influence is through relative scale, as Mansfield's work hints, or through the effects of research's role in interfirm rivalry.

VII. SUPPOSED ADVANTAGES OF SIZE OR POWER

A. Liquidity and profitability

It has been argued that since firms may be unwilling or unable to borrow substantial funds to finance development of a new product or process, only firms generating a substantial cash flow can support a sizable R&D effort. The firms with high liquidity are generally large, monopolistic firms. A closely related hypothesis suggests that high current profits, as a source of liquidity, are necessary for sizable R&D effort. Alternatively, current profits have been viewed as an indicator of future profits: a firm enjoying just success may be more inclined to take the risks of R&D in hopes and expectation of

future success. Both views support the notion that large monopolistic firms are the most likely source of technical advance because they are in the best position to reap sizable profits. A contrary hypothesis has also been suggested, namely that profits may influence the development effort inversely, since a firm whose profits are slipping may feel under pressure to innovate. To test these conjectures, several investigators have included measures of liquidity and/or profits in their regression analyses.

For a sample of 405 firms in 21 U.S. industries in 1960, Hamberg regressed the ratio of R&D personnel to total employees against profits and depreciation (a source of liquidity), sales, federal R&D contracts, gross investment, and the past scale of R&D, all deflated by gross fixed assets [31, 1966]. The fit was poor. The direction of effect of profits (as well as of sales and gross investment) varied among industries. The liquidity variable, depreciation, had little apparent influence on R&D intensity. The clearest conclusion was that past scale of R&D positively influences current R&D.

Grabowski's study of selected firms in the chemical, drug, and petroleum industries included internally generated funds along with a measure of diversification and an index of prior research productivity as explanatory variables in a regression to explain R&D expenditures per sales dollar [28, 1968]. Internal funds were measured by the sum of after-tax profits, depreciation and depletion in the previous period, all deflated by sales. The regression coefficient of the "internal funds" variable was positive and significant for each industry. The magnitude of all regression coefficients and the correlation increased with research orientation of the industry involved, being lowest in the petroleum industry and highest for the drug industry. Grabowski inferred that the more important research is as a competitive strategy in the industry, the greater the effect of the independent variables, including internally generated funds, on research intensity. Further support for this contention is provided in the Grabowski and Baxter study described about [29, 1973].

Mueller constructed and estimated, using observations of 67 firms over 1957–1960, a four-equation econometric model of the firm to explain R&D, capital investment, advertising, and dividend payment [57, 1967]. In the fitted equation for R&D intensity, the coefficient of depreciation was positive for all years, but significant only for 1958. Its greater impact on R&D in a recession year, coupled with its low coefficient for that year in the investment equation, indicated to Mueller a shift of resources from capital investment to R&D when returns to the former may have declined. Industry R&D intensity best explained firm R&D intensity.

J. W. Elliott investigated the determinants of R&D spending for 53 firms in 16 industries over 1953–1966 [19, 1971]. He was especially interested in whether profits' role in research spending decisions is primarily an "exceptional" role, as indicative of future profitability, or whether it is as a source of liquidity or funds. For each year 1957 through 1966, he regressed R&D spending per sales dollar against current and lagged R&D spending intensity, industry growth rates, firm share of market and of R&D expenditure, and relative firm size

and growth. By including or excluding each variable in the regression for each year on the basis of its statistical performance. Elliott "accounted for" most of the variation in R&D spending intensity. Lagged R&D was the only explanatory variable to appear consistently in the equations; both the sign and magnitude of its coefficients fluctuated from year to year.

Next, Elliott introduced three measures of profits (gross profits profits after taxes and dividends, sales margin over cost of goods sold) and two measures of liquidity (cash flow, discretionary income). Each was tentatively added to the regression already fit and was retained in the equation if its inclusion lowered the residual variation. The best lag specification was also selected statistically. He concluded that effects of internal profit expectations proxies tended to be more significant in general than effects of internal funds-flow variables, given the best fitting specification of other (external) variables. Funds-flow variables were judged to have a greater influence on R&D intensity when GNP was growing slowly.

Using a sample of 448 firms and a variety of statistical tests, Scherer was unable to find any significant relationship between either 1955–1960 profits or 1955 liquid assets on the one hand and 1959 patenting or 1955 R&D effort on the other [79, 1965]. The Smyth, Samuels, Tzoannos study of determinants of patented output in three United Kingdom industries found firm profitability, measured by average profits net of tax and interest payments for 1958–1963, deflated by net assets, to have no significant effect in any industry [90, 1972]. Cash flow, measured by average undistributed profits plus depreciation, had a positive coefficient in each case but it was significant for only the chemical and machine tools industries. The Johannisson-Lindstrom study of patent applications in Swedish industry found neither liquid assets (current assets less short term liabilities) nor cash flow (net profits prior to depreciation, taxes, and allocations to financial funds) especially conducive to inventive output [37, 1971]. They acknowledge possible statistical difficulties due to the very high correlation between these financial variables and firm size.

In sum, the empirical evidence that either liquidity or profitability are conducive to innovative effort or output appears slim. Grabowski has made perhaps the strongest empirical case. Yet failure to support the hypothesis may not indicate the lack of importance of this variable. Liquidity or profitability may be "threshold factors," necessary in some degree for R&D activity, but not linearly related to the amount of innovative activity. More sophisticated modeling within a multiple variable framework may be required.

B. Diversification and entrepreneurial talent

Another argument favoring the large monopolistic firm in innovation rests on its supposed diversification into many product areas. Diversification is logically distinct from both size and market power, but may be a frequent accompaniment. A firm's degree of diversification will positively influence its expected profit from R&D effort, it is argued, since a more diversified firm will be better able to utilize its research outputs. Search for a product with certain properties may yield what is sought, or it may reveal something else of potential value.

A firm doing business in a narrowly prescribed area may be unable or unwilling to produce and market a new product provided by the R&D lab but unrelated to the firm's main business. On the other hand, a widely diversified firm might utilize profitably the serendipidous finding. Since the expected profit of R&D effort increases with a firm's diversification, it is argued, R&D intensity may increase as well.

To test the hypothesis just outlined, Grabowski included in his regressions an index of diversification to explain R&D spending intensity: the number of separate 5-digit SIC product classifications in which the firm produces [28, 1968]. The regression coefficient was positive in all three industries and significant in the chemical and drug industries.

Comanor's [14, 1965] study of R&D output, the proportion of sales attributed to new products, in the pharmaceutical industry included an index of diversification reflecting the firm's participation in 40 therapeutic markets defined on the basis of apparent medical usage. Comanor found that diversification was negatively associated with R&D output, suggesting R&D effort may be more productive if it is concentrated towards a few product areas. Comanor's conclusion appears in conflict with that of Grabowski.

Scherer's investigation of patents introduced a diversification index (the number of consolidated industries in which the company operated) into regressions of patents on sales for the aggregate sample and for each of the 14 industry groups, and into a regression of R&D employment on sales [79, 1965]. The findings were mixed. The index seemed to capture the effect on companies based in industries with relatively low rates of patenting, of also operating in high-patenting industries. Scherer concluded that diversification as such does not appear necessarily favorable to patented invention. Johannisson and Lindstrom [37, 1971], employing a diversification measure in their regression to explain patent application in Sweden, reported findings very similar to those of Scherer; diversification did not seem of importance in explaining variations in inventive output, except as it reflected diversification from a low patenting industry into an industry that is more technically progressive.

Kelly employed the percent of shipments outside the firm's primary three-digit SIC category in his sample of 181 multiproduct firms as a measure of diversification [44, 1970]. He found "diversified firms more likely to invest in a higher proportion of research but the advantages of diversification for research occur for technically related products in the same two-digit SIC industry group.

In sum, the role of diversification of products in fostering or retarding innovation has been examined statistically, but without a clear conclusion. Kelly's findings may be the most promising for future research, namely that advantages of product diversification for research do exist, but mainly if the diversification is to related products.

Another supposed advantage of the large firm in innovation is that it attracts and retains the best entrepreneurial talents by offering the greatest challenges and opportunities. Since the best entrepreneurs are the most progressive, it is argued, the larger firms will tend to be the technological leaders. To test this hypothesis, Adams reasoned as follows: Americans, as a group, are widely acknowledged to be better en-

trepreneurs than Frenchman [1, 1970]. Therefore the hypothesis suggests that even moderate size firms in the United States would tend to be reasonably progressive, while the limited entrepreneurial talent of France would tend to be concentrated among the very largest firms. Hence the hypothesis that "entrepreneurship" and R&D intensity are positively related implies a greater concentration of R&D effort among large firms in France than in the United States, Adams tested the hypothesis by checking this implication. He found that in the United States, R&D spending tends to be more concentrated in large firms than is production; in France, on the other hand, large firms' share of R&D is less than their share of industry output. In sum, contrary to the prediction, small firms' role in R&D appears greater in France than in United States.

VIII. NEO-SCHUMPETERIAN ANALYSIS

While empirical tests of the Schumpeterian hypotheses were being conducted, the theory itself was undergoing another round of development. Efforts have proceeded along two routes to bridge the gap between traditional microeconomic models of competition and Schumpeter's model. Both lines of work have focused on the role of R&D rivalry in determining the pace of inventive activity and have utilized findings of previous empirical studies to guide assumptions and check conclusions. In the first groups of papers to be reviewed, R&D rivalry is supposedly confined among existing members of an industry who view each other within a Cournot oligopoly framework. In the second set of papers, the emphasis is on potential rivalry from any quarter, as stressed by Schumpeter, and requires extension of the oligopoly model along lines analogous to recent advances in the theory of limit pricing. These second generation models of innovation have already provided a basis for new empirical studies, as evidenced by the Glabowski and Baxter paper [29, 1973].

Horowitz [35, 1963] conducted an early investigation of a firms' research plans in which rival research behavior is recognized within a Cournot-like framework. He employs the textbook assumptions of linear demand and costless production to describe a representative firms' output environment. Research yields a new product, with given characteristics, whose value to the firm depends on the number of other firms also introducing it. Priority in innovation would enable a firm to capture the entire industry profits over a finite time span reflecting patent life, beyond which it reaps its ordinary oligopoly profit. The firms' conjectures regarding the research plans of rivals are expressed through a subjective probability regarding a single rival's introduction of the new product, the probability of at least two rivals introducing, and so on. Horowitz determines the maximum level of resources the firm would be willing to commit to research by balancing expected benefits against expected losses from not engaging in it. He finds that research expenditure will increase with a decline in the firm's assessed probability of rival introduction and/or the number of rivals, as well as with longer patent life. Horowitz does not analyze the speed of development decision or the welfare implications of his results.

The first attempt to deal explicity with the relationship between intensity of competition among inventors and speed of development was made by Scherer [81, 1967]. The model contains three major components: a convex time-cost trade-off between development duration and cost; profits from innovation that depend on its timing and the reaction of rival inventors; and a Cournot assumption regarding rival's plans for the introduction of a similar innovation. Scherer distinguishes between competition for a new market created through innovation and the market-sharing situation in which an innovator gains at the expense of suppliers of an existing product. Dependence of development cost on the state of technology and desired product quality is recognized but treated parametrically. The innovator captures a larger reward than an imitator, although the latter may gain at the expense of the former. The imitator's development costs are unaffected as a result of being a follower. Scherer posits that each rival is fully aware of progress made by others towards completion of development and of the belief that their rate of progress is invariant in respect to his own actions. The innovator seeks to maximize discounted net profits by selection of an introduction date while the imitator is assumed to seek a specific market share.

Employing analytic methods, specific parameter values, and simulation, Scherer derives a number of tentative conclusions:

(*a*) The development period duration varies inversely with potential reward and directly with the firm's market share.

(*b*) A firm expecting to be the leader and thereby to capture permanently a dominant market share accelerates its development in response to rival's acceleration. It decelerates its efforts if it expects to be the follower, reducing development cost.

(*c*) The inferior duopolist tends to develop its product more rapidly as innovator than as imitator, providing utility functions are linear in profit, since it stands to gain more and lose less than the dominant firm.

(*d*) The more rapidly a firm can penetrate markets through innovation, the shorter its development period. This favors firms with well established sales organizations.

(*e*) Expanding industry membership accelerates development, providing the prospect for positive profits is not eliminated through overcrowding.

All in all, Scherer's analysis suggests that rivalry stimulates rapid development of new products. It is difficult, however, to draw concrete welfare implications without taking into account losses in efficiency resulting from duplication costs and absence of perfect competition.

Evaluation of the trade-off between static efficiency and innovation has been attempted by L. E. Ruff through analysis of a simple dynamic general equilibrium model [76, 1969]. He posits an economy with a single final good (food) and a single input (labor) of limited availability. Food can be produced by a traditional method exhibiting decreasing returns to scale or by a modern constant returns to scale technique. Invention improves labor productivity in the latter method but requires ever more resources for equal successive improvements. Ruff compares resources allotted to each food production method and to research by a social planner seeking maximum present value of food

production over the indefinite future, with allocations in a decentralized economy of identical long-run profit maximizing firms. Each firm regards other firms' research activities as unresponsive to its own, a Cournot-like assumption similar to Scherer's. Varying degrees of free transmission of research results are considered. The resulting decentralized resource allocation is neither statically nor dynamically efficient by comparison with the centrally planned allocation. As the number of firms expands indefinitely, the marginal conditions for static efficiency in production are satisfied but there is no technical progress. Letting the industry shrink to a single firm results in satisfaction of the dynamic marginal conditions for positive research activity but also results in violation of the static conditions, so the allocation of resources is not socially optimal. Total discounted consumption, Ruff's posited welfare criterion, does, however, increase as the number of firms is reduced. While his analysis lends some support to the Schumpeterian view, he contends that socially optimal resource allocation can only be achieved by means of an industry-wide cooperative research lab, subsidized by government to counteract its monopsonistic behavior in the employment of labor.

W. L. Baldwin and G. L. Childs [6, 1969] employed Scherer's model to determine circumstances under which imitation is more attractive than innovation. They find imitation more desirable if it is quick and the anticipated market share is large. This might explain why large firms with established reputations are more frequently followers than leaders. Slightly slower development saves costs while procrastination does not seriously jeopardize their market position. B. Roberts and B. Holdren [73, 1972] employ several versions of Scherer's model to analyze the role of information in social welfare.

The effect of rivalry on the speed of introduction was also addressed by Y. Barzel [7, 1968]. He posits a single cost reducing innovation whose rewards (royalty revenue) accrue exclusively to its introducer. Cost reduction does not lead to price reduction so there is no change in consumer, surplus, *i.e.*, the inventor's reward coincides with society's. Since future benefits are discounted, earlier introduction increases the present value of invention. Development costs are incurred as a lump-sum at the moment of introduction so delayed introduction reduces the discounted present value of development costs. The inventor is aware of possible preemption by a rival, but the form of the interdependence is not explicit. The socially efficient introduction date is characterized by equality of marginal social cost and marginal social benefit. Barzel contends that rivalry causes socially premature introduction and thereby an overallocation of resources to inventive activity, as the inventor's fear of preemption drives the introduction date back to the zero-profit point. The zero-profit condition does not, however, lead to efficiency as in perfect competition but to inefficiency of the kind encountered when price equals average cost but not marginal cost. The absence of explicit assumption about perception of and response to rivals by the inventor makes it difficult to assess the validity of Barzel's contention; *i.e.*, it is unclear under what set of circumstances, if any, Barzel's conclusions would obtain.

Kamien and Schwartz [40, 1972] analyzed the effect of rivalry on the firm's selection of an introduction date and its relationship to the

socially desirable one. In their analysis rewards depend on whether the firm is innovator or imitator, and, if the former, whether imitators exist. The expected reward from innovation is assumed to exceed that of imitation. Early introduction increases the likelihood of being first as well as bringing the reward stream closer to the present. As in Scherer's analysis, costs of development are assumed to increase at an increasing rate with contraction of the development period. Development costs are contractual and will be incurred even if the firm loses the race for priority. The rate of resource allocation through time to development is chosen by the firm. Rivals are supposed numerous and viewed as a composite by the firm. A subjective probability distribution regarding the composite rival's introduction of a competing innovation at any date is posited for the inventor, a Schumpeterian assumption. In particular, the instantaneous conditional probability of rival introduction and of rival imitation are each assumed constant. The profit maximizing firm's introduction date may be either premature or belated relative to the cooperative solution with no rivalry for priority and thus no replication of development costs. Kamien and Schwartz also found that the expected profit-maximizing firm never selects an introduction date at which total profits are just zero. Intense competition, in which the firm disregards rivals' actions and imitation is immediate, leads to an indefinite postponement of development. Increasing rewards for innovation accelerates development, while enhancing imitation benefits has an ambiguous effect. The existence of a degree of rivalry that maximizes the rate of development is also investigated in the context of this model by Kamien and Schwartz [42, 1974]. It is found that complete absence of rivalry may be most conducive to rapid introduction of innovations in some circumstances, while an intermediate degree of rivalry is more likely than monopoly to yield the most rapid development rate when the innovator's expected quasi-rent is high. This result provides some theoretical support for the empirical finding that there exists a market structure intermediate between monopoly and perfect competition most conducive to technical advance.

In a subsequent study [43, Kamien and Schwartz, 1974] their model is extended to allow innovation to be motivated by both the prospect of monopoly profits and the fear of lost profits on the current products as a consequence of rival innovation. Moreover, development costs as well as rewards from innovation depend on whether the firm is leader or follower. The former generalization of the model allows innovative behavior of an established firm with a profitable product line to be compared with that of a new entrant or a firm in a competitive industry with no excess profits. The latter generalization permits incorporation of an important characteristic of innovation, namely that it is costly to produce and cheap to reproduce. The firm seeks maximum expected profit through selection of an introduction date and a development plan. In this model the option of revising its development plan, including discontinuance, upon precedence by a rival is available to the firm. Thus, cost of development is a stochastic variabe as a consequence or rivalry among potential innovators. Analysis of the model discloses a firm earning a monopoly profit on its current product and invulnerable to competition from a rival's

innovation is least likely to innovate, will take the longest to complete development, and is the most likely to cease development if preceded by a rival. Conversely, new entrants or firms earning no monopoly profits will be the hardest to discourage from innovating and will be the most rapid developers. A firm is less likely to discontinue development if rival precedence occurs during an advanced stage of development. The prospect of costless imitation need not retard development indefinitely providing rival imitation is not immediate.

A picture of the relationship between resource allocation and technical advance, albeit fuzzy, does emerge from these studies. The quest for profit and devotion of resources do influence the rate and direction of inventive activity despite the large role of serendipity and other goals motivating discovery. Moreover, the relationship appears bidirectional, with the state of knowledge shaping and being shaped by profit opportunities and availability of resources. The innovative process seems characterized by an almost neoclassical production structure with increasing returns up to a threshold level of resource commitment and nonincreasing returns beyond. The threshold for efficient operation constitutes an entry barrier in certain industries, though not always a very formidable one.

Having reviewed evidence for including technical change among the variables determined by an economic system, attention turned to the early theories about the merits of alternative market structures in this extended environment. The major hypothesis to emerge, founded on the assumption that innovative activity constitutes the critical form of competition, was that an industry composed of large firms with a degree of monopoly power would be the most technologically progressive. Large size and monopoly power were regarded as complementary attributes, the former influencing the breadth of the market for an innovation and the latter its duration. Subsidiary hypotheses included suppositions that large diversified firms would undertake more research than small single product firms and that large monopoly firms would attract the best innovative talent. These hypotheses, supported with anecdotes by their originators, were extremely vague regarding definitions of firm size, monopoly power, and inventive activity. The lack of specificity has led to a wide range of interpretations and dispute regarding the relevant empirical tests.

In all the papers reviewed above, development of an innovation is assumed to be a deterministic process. Yet innovative activity is commonly regarded as being subject to considerable technical uncertainty. The development plan of a firm seeking maximum expected discounted profits from innovation when the level of resources necessary for its successful completion is uncertain is characterized in [39, Kamien and Schwartz, 1971]. The combined effect of uncertainty with respect to both sucessful completion and planned rival introduction is studied in [41, Kamien and Schwartz, 1974]. The major new finding of this analysis is that uncertainty regarding rival behavior may retard spending in the early phases of an R&D project and accelerate spending in later stages. Treatment of both types of uncertainty, including interactions between the two, poses tough analytical problems and therefore has not advanced very far. Continuance of this research,

however, is essential for more complete understanding of the innovation process.

IX. SUMMARY

The survey began with a review of the empirical literature on the relationship between resource allocation to R&D and technical advance. These studies suffer shortcomings common to many empirical investigations, including those reviewed later in the paper. Choice of measures of innovational inputs and outputs is guided largely by data availability rather than a conceptual framework. Ingenuity is, however, often displayed in wringing out interesting observations from a meagre data base. Few serious attempts to cope with simultaneity elements, through modern econometric techniques, have been made.

Data availability has allowed more extensive investigation of the association of inventive activity with firm size than with market structure. A commonly tested hypothesis is that R&D activity increases more than proportionately with firm size. The bulk of empirical findings do not support it, with the notable exception of the chemical industry. Relative R&D activity, measured either by input or output intensity, appears to increase with firm size up to a point then level off or decline beyond it.

Studies of market structure and R&D activity commonly employ a concentration ratio as a measure of monopoly. Little support has been found for the standard hypothesis that R&D activity increases with monopoly power. Instead, recent evidence suggests that rivalry in R&D may be nonlinearly related to industry concentration. A new empirically inspired hypothesis has emerged to the effect that a market structure intermediate between monopoly and perfect competition would promote the highest rate of inventive activity. Some theoretical support for this has been advanced. One important defect of the empirical investigations of the relationship between either monopoly power or firm size and R&D activity is the failure to deal with the inherent simultaneities. Another is that a concentration ratio may be an unsatisfactory proxy for the extent of active rivalry in the industry. According to Rosenberg [74, 1972], traditional industrial boundaries become decreasingly useful for economic analysis as new products and processes compete across industry lines.

The hypothesized positive association between diversification and R&D activity has been weakly supported in some studies, although the direction of causality is unclear. Investigations of the supposition that large firms have the best innovative talent disclosed the opposite. The largest firms generally appear to be far less efficient innovators than smaller rivals.

Most recently, the theoretical role of priority in invention in the allocation of resources to inventive activity has been investigated. Priority is one determinant of how much of the direct benefits of the invention will accrue to its originator. (Empirically, the evidence on the advantage of priority is mixed.) From society's standpoint, the quest for priority determines how soon the fruits of inventive activity will become available and how much resources are devoted to this activity. If the cooperative solution is taken as socially desirable, then rivalry among innovators may result in either over-or-under-alloca-

tion of resources to inventive activity. It has also been shown that, within the context of a specific model, there is a degree of rivalry that results in the most rapid development of an innovation. For inventions of small value, the absence of rivalry, monopoly, leads to most rapid development, while a positive level of rivalry will achieve this for more valuable inventions. Possession of monopoly power with regard to an existing product retards innovation of a superceding product. Lastly, this work suggests attempting to measure potential rivalry in further empirical investigations of market structure and technical advance.

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SECTION II—ISSUES SURROUNDING FEDERAL R. & D. EXPENDITURES

"ECONOMIC REPORT OF THE PRESIDENT"

[Transmitted to the Congress January 1972, U.S. Government Printing Office, p. 125]

RESEARCH AND DEVELOPMENT

Investments in scientific knowledge and in its application to productive uses have become an important characteristic of the American economy. Benefits from the development and utilization of knowledge are many and varied. They are evident in improved health for millions of Americans as well as in our greater understanding of outer space. They include entirely new products that enhance the quality of life and new techniques that expand the productivity of the Nation's human and physical resources. While an accurate evaluation of those benefits that directly improve economic performance is difficult—to say nothing of the less tangible benefits—it is widely agreed that the group of activities called research and development (R&D) plays a central role in our economy. It has led to new products and industries; and it can contribute in important ways to solving today's complex economic and social problems.

Research and development has become a major economic activity. In recent years over $25 billion—nearly 3 percent of the Nation's total expenditures—has gone into R&D. Two-fifths of the expenditures for this purpose reported in 1971 were made by private profitmaking firms. The Federal Government paid for most of the remainder (Table 26).

TABLE 26.—DISTRIBUTION OF FUNDS FOR RESEARCH AND DEVELOPMENT, BY FUNDING SOURCE AND PERFORMER CALENDAR YEAR 1971

Source or performer	Percentage distribution of overall total [1]			
		Research		
	Total	Basic	Applied	Development
By funding source (total)	100.0	15.1	22.6	62.3
Federal Government	55.0	9.5	12.3	13.1
Universities and nonprofit institutions	5.3	3.7	1.3	.3
Industry	39.7	1.9	9.0	28.9
By performer (total)	100.0	15.1	22.6	62.3
Federal Government	14.6	2.5	5.3	6.8
Universities and nonprofit institutions	17.4	10.4	4.5	2.5
Industry	68.0	2.2	12.8	53.0

[1] Based on $26,900,000,000 reported by performers of R. & D. Funding and performing estimates for universities include $3,000,000,000 financed by State and local governments.

Note: Detail will not necessarily add to totals because of rounding.

Source: National Science Foundation.

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The Federal Government is itself an important performer (as well as funder) of research and development; in 1971 nearly 15 percent of all R&D was performed directly by Federal agencies. But the Federal Government's influence on the R&D industry is even larger than its actual share of these activities might imply. Government policy influences the supply of scientific manpower; it also affects incentives for private investment through cost-sharing arrangements. tax polices, patent laws, and other legal mechanisims.

Rationale for Government involvement

Government is a large purchaser of goods and services, and many of the things it buys have a large R&D component. Defense equipment and the exploration of space are obvious examples. Government as the purchaser of such goods and services must also support whatever research and development is required for their production, either through direct Federal funding of the R&D or indirectly through the price it pays for the production of the final goods themselves. The bulk of Federal expendtiures for R&D fall in this category; national defense and space alone accounted for 79 percent of R&D funding in fiscal 1971 (Table 27). Research and development done for these purposes have had applications in other fields. Therefore the amount of R&D supported for defense and space is relevant to the scale of appropriate Federal support for R&D in the private sector.

But Government has an appropriate role in R&D even when its results will not be incorporated in Government purchases. because private firms would underinvest in R&D for goods normally purchased by the *private* sector. Although an investment in R&D may produce benefits exceeding in costs from the viewpoint of society as a whole, a firm considering the investment may not be able to translate enough of these benefits into profits on its own products to justify the investment. This is because the knowledge which is the main product of R&D can usually be readily acquired by others who will compete away at least part of the benefits from the original developer. This is particularly true of basic research. where the output frequently occurs in the first instance not as a marketable product, but rather as an advance in basic knowledge that can subsequently be used in applied research and development by a wide and often unforeseeable range of firms.

One way to encourage more spending on R&D for private goods is, of course, by direct funding. When this approach is followed, it is sensible for Government's share of total expenditures to be greatest for basic research and to decline at subsequent stages. The difference between social and private benefits is largest for basic research and diminishes when investments begin to provide returns that can be obtained through private markets. Increasingly it is recognized, however, that even at the developmental, demonstration, and diffiusion stages of innovation. social benefits may exceed private benefits.

There are also indirect ways the Government can promote R&D investment for private goods. Public policy has long encouraged and rewarded innovation and the progress of science through patent laws which permit inventors to capture a larger portion of benefits than would otherwise be possible. Other legal mechanisms including those that deal with "trade secrets" also permit the entrepreneur to internalize benefits that otherwise would accrue outside his firm.

The difficulty of a firm undertaking its own R&D efforts may be especially great when the firm is small in relation to the scale required for efficient R&D efforts. In some cases this difficulty is overcome by the R&D activities of larger firms which supply machinery or materials to smaller firms, for example, by producers of farm machinery or seeds for farmers. In other cases there are firms and institutions that specialize in research and development as such. Also, firms may be able to share risks or pool their support of R&D through formal or informal consortia under today's legal and institutional arrangements. For example, in fragmented industries in which several such consortia are probable, joint R&D would not normally be considered a violation of the antitrust laws. On the other hand, joint efforts among leading firms in highly concentrated industries would normally be considered undesirable. In general, actions taken by private groups which lead to improved allocation of resources would not be in conflict with the antitrust laws; actions which lead to excessive market power would be.

It must be recognized that in some industries the small firm is the most effective institution for accomplishing R&D. This is perhaps the case most frequently at the early stages of development of a new technology. Large firms sometimes prove to be insufficiently flexible to adapt to rapidly advancing technological innovation. In other instances large, regulated firms facing relatively assured markets sometimes achieve only a slow pace of innovation. The benefits of innovation may be capturable, but the spur of competition is absent.

When private action or patent protection is not sufficient to achieve scale economies or capture external benefits, direct Government support for R&D may be appropriate. This would be especially true in an established industry with many small firms. Under such conditions an individual firm may have little incentive to undertake its own research or to participate in an ongoing venture in R&D conducted jointly by a group of other firms; it would have difficulty capturing the benefits of its own efforts, and the benefits of their efforts would probably be available without the firm's financial support. Federal support for agricultural research, for instance, started because individual farms were too small to undertake their own research and lacked the incentives and institutions to support joint arrangements.

While it is clear that Federal involvement is essential to prevent underinvestment in R&D, the optimal amount of this activity is much less clear. The proper allocation of R&D among alternative activities presents a further problem. In theory, benefit-cost analysis can answer these questions, but in practice it is difficult to measure reliably either the aggregate benefits from R&D or the benefits from investing in particular projects. This is inherent in the conditions which lead to government intervention—benefits are often widely diffused in society and thus difficult to measure. Comprehensive analysis is further hindered because the transformation of research into new knowledge and of new knowledge into public and private innovations and workable technologies is not yet adequately understood. Until better analysis is available to show the benefits, costs, and processes associated with R&D, informed judgment will continue to be the major element in shaping public policy.

Recent developments

Several recent development have raised serious questions about the adequacy of this Nation's research and development program. Recognizing these developments, the President in 1971 directed the Domestic Affairs Council to undertake an intensive review of Federal policy in this field.

The most prominent development has been in total expenditures for research and development; they grew rapidly until about the mid-1960's but have recently been rising quite slowly. Indeed, if total outlays are adjusted for rising costs, "real" outlays for R&D have actually been declining since 1968. As a result, research and development amounted to a smaller percentage of GNP in 1971 than in any year since 1960.

Federal R&D spending, in real terms, declined at an annual rate of 4 percent between its 1967 peak and 1971 principally because of scheduled reductions in space exploration from $5 to $3 billion. Nonfederal spending continued to show a real increase through 1969 and then declined the past 2 years.

New emphases

New national priorities have been reflected in substantial reallocations of Federal R&D expenditures (Table 27). All of the recent declines in total Federal outlays for R&D have been in national defense and space. National defense accounted for 86.5 percent of Federal R&D expenditures in 1960; during 1965 this proportion declined to 56.9 percent and remained near that level through 1971. The major growth in Federal support from 1961 to 1966 was in space research and technology, which by 1965 accounted for one-third of the total Federal R&D expenditures. Since 1966 space R&D has declined each year both in absolute terms and as a share of the total. Expenditures for R&D related to human resources (mainly in health and education) and economic affairs increased rapidly throughout the decade. Between 1965 and 1971 the share of the total devoted to these fields doubled; together they accounted for 18.6 percent of Federal R&D expenditures in 1971.

TABLE 27.—DISTRIBUTION OF FEDERAL EXPENDITURES FOR RESEARCH AND DEVELOPMENT BY MAJOR FUNCTION, FISCAL YEARS 1960, 1965, AND 1971

[Fiscal years]

Function	Percentage distribution		
	1960	1965	1971 [1]
Total	100.0	100.0	100.0
National defense	86.5	56.9	57.7
Space research and technology	4.7	33.0	21.6
Human resources [2]	5.0	6.3	13.0
Natural resources and environment	1.0	1.0	1.7
Economic affairs [3]	2.6	2.5	5.6
Other [4]	.1	.1	.3

[1] Estimate.

[2] Health; education, and manpower; income security; veterans benefits and services; and community development and housing.

[3] Commerce and transportation; and agriculture and rural development.

[4] International aflairs and finance; and general government.

Note: Detail will not necessarily add to totals because of rounding.

Source: National Science Foundation.

Unemployed scientific manpower

Declines in "real" research and development expenditures, especially the shift in Federal programs away from defense and space, and a slowing of general economic activity have increased unemployment among the Nation's scientific workers. Statistics available on the extent of this unemployment indicate that nationally it is lower than overall unemployment; but it is clear that for certain skills and in certain localities unemployment has become a severe problem, especially in contrast to the tight supply situation only a few years ago. The amount of actual unemployment, however would probably not indicate the full extent to which scientific personnel are underutilized, since some people are employed at jobs which do not fully use their technical skills.

International developments

During the last decade the United States has devoted a larger share of its GNP to research and development than any other country and a larger portion of these dollars to basic research, the type that provides the greatest external benefits. Experience of recent years has demonstrated that the benefits of R&D go beyond the borders of the performing nation. Basic research findings from all parts of the world are generally available for all nations to use, and the same increasingly appears true for applied and developmental research efforts as well. This has become evident in the shortened period between the time a new product is introduced and the time it is replaced by newly developed competitive products.

These developments are in part a natural result of expanding national economies throughout the world and of improved networks of international communications. They also result partly from specific policies of some nations to import the findings of basic and applied research conducted elsewhere and to concentrate domestic efforts on developing and refining applications.

These and other trends in national and international R&D policies have implications for the international competitive position of U.S. exports, which have been concentrated in high-technology goods, dependent on R&D investments. The conditions which underlay this Nation's comparative advantage in such goods in the past no longer appear so prominent. Both the level and the mix of U.S. research and development have changed considerably in recent years. The level of all R&D as a percentage of GNP in the 1970's may remain below that of the 1960's. In many other industrial nations the reverse would appear likely. The nature of R&D activities will help determine tomorrow's comparative cost conditions and the patterns of world trade.

Expanded support for R&D

The President's budget for fiscal 1973 proposes an increase in R&D funding of $1.4 billion, or 8 percent, above fiscal 1972. This increase should help reverse the recent declines in "real" Federal funding for all R&D activities. Federal support is being expanded in several critical areas: basic research; national security; and civilian R&D. In addition, the Administration is moving to improve the overall management of R&D to ensure an appropriate level, priority, and efficiency of effort.

The Budget calls for a 15 percent ($700 million) increase for civilian R&D. Over one-half of the increase will be directed toward six priority domestic objectives. New emphasis will be given to potentially fruitful developments in the fields of energy, environment, transportation, health, natural disasters and drugs. In addition, two experimental programs will be initiated to stimulate R&D investments and applications by private firms and nonfederal institutions. One program will be administered by the National Bureau of Standards; the other program, to be administered by the National Science Foundation, will include efforts to help improve our understanding of the process of innovation and research application. A variety of approaches will be followed by both agencies including more joint activities among universities, industry and Government, demonstration of new technologies and encouragement for small, innovative R&D firms.

Science Indicators 1974

Report of the National Science Board 1975

National Science Board
National Science Foundation

Letters of Transmittal

Message from the President of the United States transmitting the Seventh Annual Report of the National Science Board, Pursuant to Section 4(g) of the National Science Foundation Act, as Amended, February 23, 1976. (*Science Indicators 1974*)

TO THE CONGRESS OF THE UNITED STATES:

I am pleased to submit to the Congress the Seventh Annual Report of the National Science Board entitled, "Science Indicators 1974." It has been prepared in accordance with Section 4(g) of the National Science Foundation Act, as amended by Public Law 90–407.

This report is a part of a continuing effort by the National Science Board to develop a statistical and comparative picture of the status of American science and technology. On balance, the data in this report and other evidence indicate that the Nation's research and development enterprise continues to be productive and competitive. The report also shows the unfortunate fact that inflation and the recent recession have affected adversely the level of effort and the resources that are devoted to the Nation's research and development activities—much the same as other programs have been affected. Fortunately, we are making solid progress in correcting these problems and the prospects for the future are very good.

The Nation's research and development efforts are important to the growth of our economy, the future welfare of our citizens, and the maintenance of a strong defense. The Nation's must also have a strong effort in basic research to provide the new knowledge which is essential for scientific and technological progress. My 1977 Budget now before the Congress reflects my views on the importance of science and technology in achieving our national objectives.

I commend this report to your attention.

Gerald R. Ford.

The White House, *February 23, 1976*

Resources for Research and Development

INDICATOR HIGHLIGHTS

- National expenditures for research and development (R&D) in the United States increased in current dollars each year between 1960-74, reaching $32 billion in 1974; in constant dollars, however, expenditures remained at $22-23 billion between 1968 and 1974.
- The total number of (full-time equivalent) scientists and engineers engaged in R&D reached its highest level in 1969 (at 558,000) and declined to almost 528,000 in 1974; the decline is due largely to reductions of such personnel in industry as a result of cutbacks in Federal funds in the aerospace area.
- The fraction of the gross national product (GNP) going to R&D declined steadily from a high of nearly 3.0 percent in 1964 to a low of 2.3 percent in 1974; Federal funds for R&D, as a fraction of GNP, dropped from 2.0 to 1.2 percent between 1964 and 1974, whereas funds from all other sources combined remained at approximately 1.0 percent of GNP throughout the period.
- Federal funds for R&D increased in current dollars in all but two of the years between 1960-74, reaching their highest level of nearly $17 billion in 1974; funding in constant dollars, however, peaked in 1966 and was down by 19 percent in 1974 to less than $12 billion, which is equivalent to the funding level of 1963.
- R&D funds provided by industry rose more rapidly than those of the Federal Government during the 1960-74 period, reaching nearly $14 billion in current dollars in 1974; funds in constant dollars were at their highest level in 1973, some 2 percent above the level of 1974.
- The Federal Government and industry provided 96 percent of all the funds for R&D in 1974; the Federal share of the total declined from a high of 65 percent in 1965 to a low of 53 percent in 1974, while industry's share grew from 33 to 43 percent of the total.
- R&D expenditures increased in current dollars in all R&D-performing sectors[1] in recent years, whereas funds expended in constant dollars were lower in each sector in 1974 than in previous years; the largest constant dollar decline was in industry where expenditures in 1974 were 9 percent lower than in 1969, due largely to declines in Federal support for industrial R&D.
- The proportion of R&D funds allocated to different types of R&D activities—basic research, applied research, and development—has remained nearly constant since 1965, with development receiving 64 percent, applied research 23 percent, and basic research 13 percent.
- R&D funds provided by the Federal Government are a declining fraction of the total Federal budget, falling from a high of 13 percent in 1965 to 7 percent in 1974; as a fraction of the "relatively controllable" portion of the Federal budget,[2] R&D spending has changed little, at 15 percent in 1974 compared with a high of 16 percent in 1967 and a low of 14 percent in 1970.
- Federal funds for R&D go primarily to national defense, with "civilian"[3] areas and space exploration receiving the remainder; the proportion of total Federal R&D funds

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[1] The sectors included are industry, Federal intramural laboratories, universities and colleges with their Federally Funded Research and Development Centers, and other nonprofit institutions.

[2] That part of the budget which is subject to annual appropriations, rather than determined by fixed costs and "open ended" programs whose funds increase by law.

[3] Includes areas such as health, energy, and the environment; see figure 2-10 for a listing of the areas.

for defense remained at slightly more than 50 percent throughout 1969-74, whereas the fraction for civilian areas rose steadily from 24 to 34 percent while the share for space R&D declined from 24 to 14 percent.

- Funds from the Federal Government for civilian R&D increased 70 percent in current dollars and 28 percent in constant dollars between 1969 and 1974; the civilian fields accounting for most of the growth were health (39 percent of the total growth) and the environment (17 percent).
- Federal funds for civilian R&D are concentrated on research (applied and basic) rather than development—in contrast to defense and space R&D; in 1974, 72 percent of the funds went for research, with 45 percent going for applied research and 27 percent for basic research.
- Federal funds for laboratory equipment provided through research grants declined as a fraction of total grant funds, decreasing from 11 percent in 1966 to 5 percent in 1974.[4]
- Federal support for major fixed equipment and R&D facilities in 1974 was well below the years of highest funding in the mid-1960's even though such support has increased considerably since 1972.
- Expenditures by the Federal Government for the dissemination of the results of R&D increased in current dollars each year from 1960 through 1974, but changed little in constant dollars after 1968; the ratio of these obligations to total Federal obligations for R&D has remained at approximately .025 since 1970.

Basic Research

INDICATOR HIGHLIGHTS

- The Nation's total expenditures for basic research rose continually during the 1960-74 period in current dollars; in constant 1967 dollars, funds for basic research in 1974 were equal to the 1965 level, and almost 13 percent lower than the peak year of 1968.

- Universities accounted for approximately 55 percent of the Nation's total expenditures for basic research in 1974 (versus 37 percent in 1960), followed by the Federal Government and private industry at some 15 percent each, and other sectors with the remainder.

- The Federal Government provided the largest share of support for basic research during the 1960-74 period, increasing from nearly 60 percent of all such funds in 1960 to almost 70 percent in 1974; industry's share declined from 28 percent in 1960 to 15 percent in 1974, and the universities' share increased from 6 to 11 percent over this period.

- Funds provided by the Federal Government for basic research increased each year (except for 1971) in current dollars, but declined 13 percent between 1968 and 1974 in constant dollars; the largest reductions in constant dollars were recorded in the physical sciences which declined approximately 25 percent between 1969 and 1974.

- University expenditures for basic research (from all sources of support) rose continuously in current dollars between 1960-74, but declined some 5 percent in constant dollars between 1968 and 1974; this decline is due to reduced growth of Federal support in combination with inflation.

- Basic research expenditures by academic institutions in 1974 were concentrated in the life sciences (51 percent of all expenditures), engineering (12 percent), physical sciences (13 percent), social sciences (8 percent), and the environmental sciences (7 percent).

- Federal support for basic research in universities, which accounted for 70 percent of all such funds in 1974, increased in current dollars between 1964-74 in the broad fields of science and engineering; the level of research effort as reflected by constant dollar expenditures, however, was lower in each field in 1974 than in previous years, with the largest reductions occurring in engineering and the physical sciences.

- Federal support for universities in 1974 was provided primarily through six agencies—NSF, HEW, DOD, USDA, AEC, and NASA—with no more than two agencies supplying at least 70 percent of all Federal basic research support in each major field of science; the NSF provided either the largest or second largest amount of funding among these agencies in each field.

- Expenditures for basic research per scientist and engineer in doctorate-granting institutions were almost 30 percent lower in constant dollars in 1974 than in 1968; the largest decline was in physics, where reductions were nearly 40 percent from 1966 to 1974.

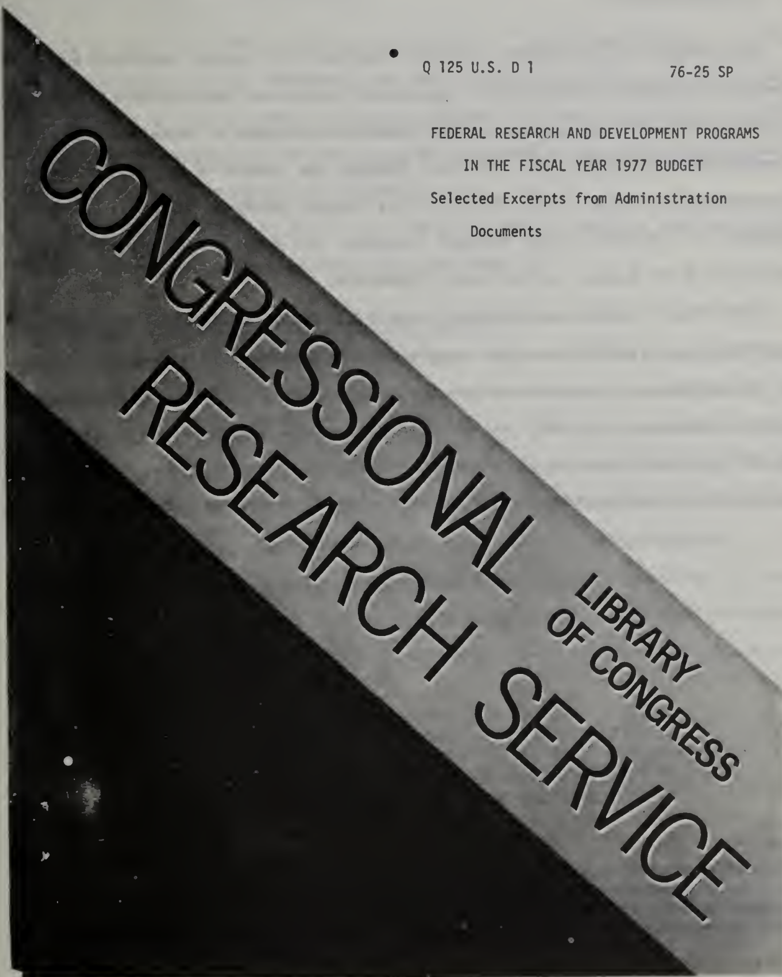
- Federal laboratories accounted for 16 percent of the total national expenditures for basic research in 1974; current dollar expenditures by these laboratories increased throughout most of the 1960-74 period, but the level of research effort in terms of constant dollars was some 20 percent lower in 1974 than in 1970, the year of highest real expenditures.

- Private industry was responsible for 16 percent of the total national expenditures for basic research in 1974; although current dollar expenditures have risen, particularly since 1972, inflation reduced real expen-

ditures in 1974 to approximately the same level as 1961.

- The number of research publications from major fields of science increased generally throughout the 1960's, but leveled off in several fields in the early 1970's; publication output in chemistry, engineering, and physics, for example, has remained at a nearly constant level in recent years.
- Universities are by far the largest producers of published research reports with some 75 percent of the total in 1973, followed by the Federal Government and private industry with approximately 10 percent each, and other nonprofit institutions with 5 percent.
- Basic research contributes increasingly to technological innovation, as reflected by the growing number of citations to research in patents associated with major advances in technology; the frequency of such citations increased 17 percent between the 1950's and 1960's, while citations to other patents declined by almost 25 percent.
- Research performed in universities is most frequently cited as the origin of patented technological advances, accounting for almost 55 percent of the cited research in recent years and replacing industry as the prime sector in which such research is performed.

Excerpts were selected from the following document which provides an excellent summary of Administration papers related to the Fiscal Year 1977 Research and Development Budget. Tables and Documents referred to but not incorporated in the Committee Print are available in Committee files.



FEDERAL RESEARCH AND DEVELOPMENT PROGRAMS
IN THE FISCAL YEAR 1977 BUDGET
Selected Excerpts from Administration
Documents

FEDERAL RESEARCH AND DEVELOPMENT PROGRAMS IN THE FISCAL YEAR 1977 BUDGET:
Selected Excerpts from Administration Documents

I. INTRODUCTION

The purpose of this report is to pull together into a single reference source summary information concerning the requested research and development programs of the departments and agencies of the Federal Government as contained in recent Presidential statements, the Economic Report, the Budget of the United States Government for Fiscal Year 1977, and the Special Analyses volume. 1/ Also included is a report on the Federal research and development program from the Office of the Science Adviser to the President.

This report consists primarily of selected excerpts which can serve both as secondary source materials as well as guides to related materials not here included. No attempt has been made to analyze the content of the individual department and agency programs; these will be found in discussions which have been and will be appearing in the press in coming weeks, and will be the focus of the authorization and appropriations hearings in the Congress which have already begun.

1/ Details concerning major programs which constitute line items in the budgets of the departments and agencies are contained in the Appendix volume of the Budget. These have not been included in this document, because of space and time limitations.

II. RELEVANT INFORMATION IN RECENT PRESIDENTIAL STATEMENTS AND THE ECONOMIC REPORT

In the past, the State of the Union Message, the Budget Message, and the Economic Report of the President, as well as the Annual Report of the Council of Economic Advisers have, in varying degrees, focused on Federal research and development programs as instruments to contribute to the realization of broader national and international objectives.

The following excerpts from the current documents provide limited insight into Presidential thinking regarding these activities. 2 /

A. State of the Union Message of January 19, 1976

The theme of this message was a proposal that the Nation focus on a "new realism" and that "new balances" must be struck--between the individual and the government, within our system of federalism, and between domestic and defense spending. President Ford alluded to the role of science and technology in only one area: Energy. Several of the energy proposals the President urged Congress to act on can only be achieved through further research and development, e.g., the development of more and cleaner energy from coal, the expediting of clean and safe nuclear production, and the acceleration of the development of technology to utilize solar energy. The relevant portion of his address is printed below:

> Taking a longer look at America's future, there can be neither sustained growth nor more jobs unless we continue to have an assured supply of energy to run our economy. Domestic production of oil and gas is still declining. Our dependence on foreign oil at high prices is still too great, draining jobs and dollars away from our own economy at the rate of $125 per year for every American.

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2 / For purposes of this document, science, engineering, and technology and research and development are used interchangeably. In point of fact, as the excerpts indicate, there was little direct reference to any of these terms in Presidential statements.

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Last month, I signed a compromise national energy bill which enacts a part of my comprehensive energy independence program. This legislation was late, not the complete answer to energy independence, but still a start in the right direction.

I again urge the Congress to move ahead immediately on the remainder of my energy proposals to make America invulnerable to the foreign oil cartel.

My proposals, as all of you know, would:

—reduce domestic natural gas shortages;
—allow production from Federal petroleum reserves;
—stimulate effective conservation, including revitalization of our railroads and the expansion of our urban transportation systems;
—develop more and cleaner energy from our vast coal resources;
—expedite clean and safe nuclear power production;
—create a new national Energy Independence Authority to stimulate vital energy investment; and
—accelerate development of technology to capture energy from the Sun and the Earth for this and future generations.

3/

B. Budget Message of the President, January 21, 1976.

The Budget Message began:

The Budget of the United States is a good roadmap of where we have been, where we are now, and where we should be going as a people. The budget reflects the President's sense of priorities. It reflects his best judgment of how we must choose among competing interests. And it reveals his philosophy of how the public and private spheres should be related.4/

Continuing on the underlying theme of the "balance" from the State of the Union Message, Mr. Ford enumerated areas in which he had tried to sort out the priorities to achieve "fairness and balance":

—between the taxpayer and those who will benefit by Federal spending;
—between national security and other pressing needs;
—between our own generation and the world we want to leave to our children;

3/ U.S. President. (Gerald R. Ford, 1973--). The State of the Union. Delivered before a Joint Session of the Congress, Jan. 19, 1976. Weekly Compilation of Presidential Documents, v. 12, Jan. 23, 1976: 43-52. At p. 47.

—between those in some need and those most in need;
—between the programs we already have and those we would like to have;
—between aid to individuals and aid to State and local governments;
—between immediate implementation of a good idea and the need to allow time for transition;
—between the desire to solve our problems quickly and the realization that for some problems, good solutions will take more time; and
—between Federal control and direction to assure achievement of common goals and the recognition that State and local governments and individuals may do as well or better without restraints.

4 /

Proceeding from the abstract to the specific, President Ford identified defense and energy needs as among the Nation's highest priorities: 5 /

> Clearly, one of the highest priorities for our Government is always to secure the defense of our country. There is no alternative. If we in the Federal Government fail in this responsibility, our other objectives are meaningless.
>
> Accordingly, I am recommending a significant increase in defense spending for 1977. If in good conscience I could propose less, I would. Great good could be accomplished with other uses of these dollars. My request is based on a careful assessment of the international situation and the contingencies we must be prepared to meet. The amounts I seek will provide the national defense it now appears we need. We dare not do less. And if our efforts to secure international arms limitations falter, we will need to do more.
>
> Assuring our Nation's needs for energy must also be among our highest priorities. My budget gives that priority.
>
> While providing fully for our defense and energy needs, I have imposed upon these budgets the same discipline that I have applied in reviewing other programs. Savings have been achieved in a number of areas. We cannot tolerate waste in any program.

Since realizing defense and energy needs requires research and and development, we can only infer that the President's message accords priority to defense and energy research and development also.

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4 / The Budget Message of the President. In U.S. Executive Office of the President Office of Management and Budget. The Budget of the United States Government, Fiscal Year 1977. Washington, U.S. Govt. Print. Off., 1976. 385 p. At pp. M3, M4.

5 / Ibid, pp. M4-5.

C. Economic Report of the President, January 26, 1976.

The message to the Congress accompanying the Annual Economic Report identified three main goals for the economic program for calendar 1976:

> First, a long-term continuation of the effort to revive the American economy; second, implementation of the many programs necessary to provide cushions for the unemployed during the transition to a healthy economy; and third, the elimination of Government policies and institutions that interfere with price flexibility and vigorous competition. 6 /

In discussing the first of these goals, the President stressed the importance of increased capital investment by the private sector if full employment and noninflationary growth--our long-range goals--are to be met. This must be accompanied by a slowing down of Federal spending in the immediate future. In his own words:

> Increased capital formation is essential to meeting our long-term goals of full employment and noninflationary growth. Although there is no shortage of industrial capacity at the present time, many of our current priorities—to become independent in energy, to improve the environment, to create more jobs, and to raise our living standards—require increased investment. This means that business investment in plant and equipment as a share of gross national product must increase. We must also slow the growth of Federal spending in the years immediately ahead, so that mounting claims by the Federal Government on our economic resources will not prevent an adequate flow of savings into capital investments. 7/

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6 / U.S. President (Gerald R. Ford, 1973--). Economic Report of the President Transmitted to the Congress January 1976, together with The Annual Report of the Council of Economic Advisers. Washington, U.S. Govt. Print. Off., 1976. 282 p. At p. 4.

7 / Ibid, p. 5.

The President's Economic Report concluded with a discussion of U. S. energy policy and the need of the Nation to reduce its dependency on Middle East oil in order to provide a "sound basis for international cooperation in the development of new fossil fuel and other energy sources." 8 / In the review of actions to achieve greater energy self-sufficiency which followed, the Federal research and development effort required was not mentioned.

D. Annual Report of the Council of Economic Advisers

The economic course recommended by the Council of Economic Advisers for the coming year in its Annual Report is to pursue policies which are consistent with "sustainable long-term noninflationary growth and to try to limit the size and duration of any policy deviations that promise short-term benefits but risk interfering with our long-run goals." This course of action appears to offer the "safest and surest route to full employment in future years." This concept is enunciated in the following summary paragraph:

> What is called for in our judgment is a steadier course in macroeconomic policies than has been followed in the past. We should set policies broadly consistent with sustainable long-term noninflationary growth and try to limit the size and duration of any policy deviations that promise short-term benefits but risk interfering with our long-run goals. The severity of the recent recession does call for maintaining stimulative economic policies to accommodate an expansion of real output at a rate above that sustainable in the long run. But departures from the policies that are appropriate in the long run should be moderate. If we do not commit ourselves to a gradual recovery over a period of years, we may increase economic instability and lose our chance for sustainable growth, which we believe offers the safest and surest route to full employment in future years.
>
> 9 /

8 / Ibid, pp. 7-8.

9 / U.S. Council of Economic Advisers. Annual Report. In Economic Report of the President Transmitted to the Congress, January 1976. op. cit., p. 21.

Private Capital Requirements Needed to Achieve 1980 Goals. The importance of increased capital investment by the private sector in order to meet our long-range goals was noted in the President's Economic Report. The Council of Economic Advisers considered the capital formation issue from the point of view of "whether the level of investment spending expected under current conditions is likely to be adequate for the attainment of certain longer-term economic and social objectives, such as full employment, greater energy independence, and a cleaner environment." 10/

To help answer this question, the CEA commissioned the Bureau of Economic Analysis of the Department of Commerce to conduct a study of the capital investment needed to produce an output which can be expected to achieve full employment in 1980. 11/ The study which was completed the latter part of 1975 showed that a ratio of business investment to GNP as low as 9.9% during the 1970's could achieve the output levels needed to meet the 1980 objectives but only if the ratio of capital investment to output remained at the level of 1970, and if additional investment requirements attributable to changing technology in certain industries and to meeting national goals relating to pollution abatement and greater energy independence are not taken into account. Because these additional requirements must be factored in, the BEA analysis estimated that a business investment ratio of 11.4% of the GNP will be required.

Table 5 and a summary explanation of the information it displays have been excerpted from the CEA annual report. Also included is a paragraph which suggests that a higher ratio than that indicated in the Department of Commerce study may be needed.

10/ Ibid, p. 40.

11/ Ibid, p. 45.

TABLE 5.—*Factors affecting the cumulative total business fixed investment required from 1971 through 1980, by major industries*

[Billions of 1972 dollars]

Factor	Total	Agriculture, forestry, and fisheries	Mining	Construction	Manufacturing	Transportation	Communication	Electric, gas, water, and sanitary services [1]	Services [2]	Other [3]
Fixed 1970 capital-output (c/o) ratios, pollution control requirements limited to pre-1970 law	1,283.4	68.5	48.5	29.5	292.2	134.7	101.1	209.5	173.8	225.7
Add for actual Pollution Control Laws passed in 1970 and 1972	47.8	....	.9	.5	29.5	.6	.0	14.2	.3	1.8
Add for industries with c/o ratios increasing for reasons other than the achievement of greater energy independence	118.2	10.3	4.2	.0	35.3	5.3	.4	.4	62.4	.0
Add for industries with decreasing c/o ratios	−36.0	−.0	−21.8	−.0	−13.2	−.0	−.0	[4] −1.0	−.0	−.0
Add for additional capital required for greater energy independence	57.9	.0	49.0	.0	.0	.0	.0	8.9	.0	.0
Add for increase in pollution control investment induced by additional investment in energy	2.0	.0	.4	.0	[5] .2	.0	.0	1.3	.0	.0
Total business fixed investment required	1,473.4	78.8	81.2	30.0	344.0	140.6	101.4	233.3	236.5	227.5

[1] Includes production by both public and private enterprises.
[2] Consists of hotels and lodging places, personal and repair services, business services, automobile repair and services, amusements and medical, educational services and nonprofit organizations.
[3] Consists of wholesale and retail trade and finance, insurance and real estate.
[4] Increase in discard rate in gas utilities due to energy considerations would produce this decline unless offset by $1.0 billion higher investment required for greater energy independence.
[5] Although the outputs and capital-output ratios of petroleum refining and related industries are not assumed to change in the process of achieving greater energy independence, the substitution of lower-grade domestic crude for higher-grade imported crude causes some additional pollution control expenditures in petroleum refining.

Note.—Detail may not add to totals because of rounding.

Source: Department of Commerce, Bureau of Economic Analysis.

(p. 45)

As shown in Table 5, there are three major reasons for the need to devote an increased share of GNP to fixed investment:

1. Investment in pollution abatement equipment as a consequence of legislation relating to "clean air" and "clean water" is estimated to add about $48 billion (1972 dollars) to the base level 1971–80 investment total. This base level, which is estimated on the assumption of fixed capital-output ratios in all industries, is identified as "pre-1970 law" in Table 4. Less than half of this additional requirement is believed to have been met by 1975.

2. Changing technology in selected industries, such as agriculture, ferrous mining and nonferrous metals manufacturing, communication equipment manufacturing, transportation, business services, and auto repair, in all of which capital-output ratios have been increasing, is estimated to add about $118 billion to the cumulative investment required from 1971 to 1980, while industries with declining capital-output ratios subtract about $36 billion.
3. To meet the goal of greater energy independence, increased investment in petroleum mining, electric utilities, and other energy-related industries is required. This is estimated to add about $58 billion to the 1971–80 investment total. Another $2 billion is required for the induced increase in pollution control expenditures by energy-producing or processing industries. If the decline in the capital-output ratio of petroleum mining continues, the cumulative investment could be $21.8 billion less. Any further decline in capital-output ratios in petroleum mining, however, would be inconsistent with the assumption of increased domestic energy output.

INFERENCES

Although these estimates are by no means definitive, they do allow some cautious inferences. Because the ratio of business fixed investment to GNP in 1971–74 continued at the 10.4 percent level that prevailed from 1965 to 1970, the business fixed investment to GNP ratio may have to average 12 percent from 1975 to 1980 to meet the capital requirements projected for 1980. Since investment is expected to amount to less than 10 percent of GNP in 1975–76, these estimates suggest that investment ratios even higher than 12 percent may be necessary in the next 4 years to put enough capital in place by the end of 1980 to meet the goals previously stipulated. (pp. 45-46)

III. FEDERAL RESEARCH AND DEVELOPMENT PROGRAMS IN THE BUDGET DOCUMENTS

The total of all Federal research and development programs for which funding has been requested for Fiscal Year 1977 is estimated at $23.5 billion, with an additional $1.2 billion for research and development facilities, making a grand total of $24.7 billion. Outlays for these activities are estimated to total $22.9 billion for the conduct of research and development and $.6 billion for facilities--$23.5 billion in all. The estimated outlays are expected to constitute just under 6 per cent of the $394.2 billion Federal budget. This percentage is about the same for Fiscal Year 1976 and slightly less than in Fiscal Year 1975.

Federal research and development details are presented in the Budget documents under three major classifications:

1. By function. In the Budget proper and also in the new Current Services Budget 12/, all Federal activities are classified under one of the fifteen functional headings, according to their single most important purpose.

2. By Department or Agency. In the Appendix to the Budget which contains details concerning research and development activities of each organizational unit when these are extensive enough to warrant line item status.

3. By Department or Agency and by Government-Wide Programs. The Special Analyses volume contains aggregated information on research and development in

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12/ Section 605 of the Congressional Budget and Impoundment Control Act of 1974 requires that the President submit to the Congress the estimated outlays and proposed budget authorities which would be included in the Budget for the ensuing fiscal year if all programs and activities were continued without policy changes. The data are to be presented according to functional classifications, by major programs within each such function, and by agency, and shall be accompanied by economic and programmatic assumptions used in arriving at estimated outlay and budget authority levels.

The first "Current Services" budget was transmitted to the Congress November 10, 1975. U.S. Office of Management and Budget. Current Services Estimates for Fiscal Year 1977. Communication from the Director, Office of Management and Budget, Executive Office of the President, transmitting the Current Services Estimates for Fiscal Year 1977... Washington, U.S. Govt. Print. Off., 1975. 63 p. [at head of title: 94th Congress, 1st Session. House Document No. 94-306]

Special Analysis P, Federal Research and Development Programs. Additional information is found in special analyses of the several social program areas--education (Special Analysis I), health (Special Analysis K), civil rights activities (Special Analysis M), crime reduction (Special Analysis N), and in environmental programs (Special Analysis Q).

A. Research and Development in the Federal Program by Functions.

Federal outlays have been presented in the Budget on a functional basis since 1948, but these classifications have taken on new importance since the enactment of the Congressional Budget Act of 1974. This act directs that the functional structure form the basis for congressional consideration of the budget and the setting of budget targets and ceilings. The law does not require that the functional classifications be those which are in the Budget but for the present they are the ones being used.

> While budget outlays have been presented on a functional basis since 1948, this classification has taken on new importance with enactment of the Congressional Budget Act of 1974 (Public Law 93-344). Under terms of this act, Congress is now required to adopt at least two concurrent resolutions on the budget each year. The first resolution is to provide targets for budget authority and outlays by major function; the process of authorizations and appropriations is to work in tandem with the first concurrent resolution to ensure that congressional action on specific appropriations and authorizations is consistent with the overall targets. The second concurrent resolution adjusts the overall targets and converts them into firm ceilings, including any directions to congressional committees necessary to meet these ceilings. As a result, the functional structure is now being used as a basis for budget review as well as a means of displaying budget information. 13/

13/ The Budget of the United States Government, Fiscal year 1977. op. cit. p. 55.

An overall array of budget functions and subfunctions, giving comparable information over the ten-year period, 1968-1977 is found in Table 19 from the Budget. It is reproduced below. It is possible to identify research and development as subfunctions in a number of areas. However, there are certain problems with this classification as a means of identifying research and development activities. One is the fact that not all functions contain a research and development subfunction, probably because the amounts in some cases are not large enough to factor in a table whose base is billions of dollars. Another is the fact that certain subfunctions combine research with other related activities, e.g., research and education, or lump energy research and development in a general heading entitled "Energy", even though the former activity constitutes an overriding proportion.

The descriptions of the various Federal functions in Part 5 of the Budget proper contain additional information concerning research and development activities. Reproduced below are the entire description of the General Science, Space, and Technology function and relevant portions of the National Defense; Health; and Natural Resources, Environment, and Energy functions. An excerpt from the General Government function is included because it relates to the Office of Science and Technology Policy which is expected to be established by legislation in the Executive Office of the President in 1976.

NATIONAL DEFENSE

EXCERPTS: PP. 61, 68, 70

The national defense function includes the funds to develop, maintain, and equip the military forces of the United States and to provide military assistance to foreign governments.

Program Highlights

- Continue the increase begun in 1976 that reversed the 7-year decline in real defense resources.
- Further modernize strategic and general purpose forces to deter nuclear and conventional attacks.
- Improve the readiness, combat effectiveness, and structure of general purpose forces within current personnel levels.
- Reduce programs that do not contribute directly to combat effectiveness, and lower civilian personnel levels accordingly.

• • •

Research and development.—Technological superiority of U.S. forces depends upon adequate investment in research and development. To maintain this superiority, recommended total obligational authority will increase to $10.5 billion in 1977, $1.8 billion above the 1976 level.

Strategic weapon systems development will continue on the B–1 aircraft, the Trident submarine and missile system, a new intercontinental ballistic missile system, strategic cruise missiles and warhead improvements, as will research on ballistic missile defense technology.

Research and development activities will also continue the major modernization of general purpose forces started in previous years. The Army development program includes a new tank, infantry combat vehicle, attack and transport helicopters, and air defense system. The Navy will develop the F–18 air combat fighter to complement the sophisticated F–14 fleet defense aircraft. The Navy will also continue development of improved fleet air defense and antisubmarine systems. Full-scale development of a tactical cruise missile will lead to a more effective attack capability for ships.

The Air Force will continue development of the F–16 air combat fighter. In addition, work will proceed on systems capable of neutralizing enemy air defenses and on exploration of the combat potential of high-energy lasers and vehicles piloted by remote control. Funding for a major new aeropropulsion systems test facility will be provided in 1977. This facility will be required for the development and testing of advanced military aircraft engines and will result in substantial future savings in the development costs of such engines. (68)

• • •

Atomic energy defense activities.—Nuclear weapons research, development, underground testing, and production activities are expected to remain at about 1976 levels. Additional funds are requested for safety, environmental, and waste storage improvements as well as cost increases. The physical security of nuclear weapons and nuclear materials at Government sites will continue to be improved. (70)

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GENERAL SCIENCE, SPACE, AND TECHNOLOGY

The general science, space, and technology function includes the space research and technology programs of the National Aeronautics and Space Administration (NASA), the physical science programs of the Energy Research and Development Administration (ERDA), and all activities of the National Science Foundation (NSF). Outlays for general science, space, and technology are estimated at $4.5 billion in 1977, an increase of $196 million over 1976, and $4.6 billion in 1978.

Program Highlights

- Continue development and testing of the space shuttle, but defer procurement of a third space shuttle vehicle in recognition of the need for fiscal restraint.
- Improve technologies for surveying natural resources and weather forecasting from space.
- Strengthen the Federal Government's overall support for basic science through programs of the National Science Foundation and the Energy Research and Development Administration.
- Provide for construction of a positron-electron colliding beam facility in high energy physics to develop and test new theories on the ultimate nature of matter.

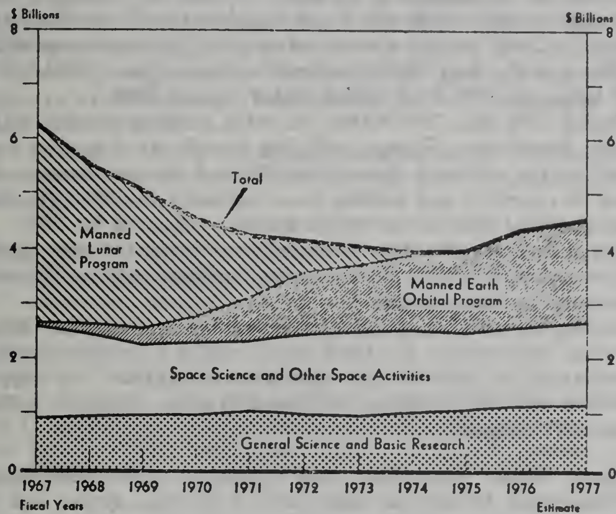
GENERAL SCIENCE, SPACE, AND TECHNOLOGY

[In millions of dollars]

Program or agency	Outlays				Recommended budget authority for 1977 [1]
	1975 actual	1976 estimate	TQ estimate	1977 estimate	
Space research and technology:					
Manned space flight	1,535	1,735	469	1,865	1,891
Space science, applications, and technology	1,084	1,118	281	1,125	1,087
Supporting space activities	334	337	80	349	355
Subtotal	2,953	3,190	830	3,339	3,333
General science and basic research:					
National Science Foundation	662	721	221	734	805
Energy Research and Development Administration	374	400	107	434	481
Smithsonian Institution	2	2	1	2	2
Subtotal, general science and basic research	1,038	1,124	328	1,170	1,288
Deductions for offsetting receipts	−2	−3	−1	−2	−2
Total	3,989	4,311	1,157	4,507	4,618

[1] Information on budget authority for 1975, 1976, and the transition quarter is shown in table 14 of Part 8.

Outlays for General Science and Space



Activities in this function are only a small part of the Federal Government's support of scientific research and development. Most of this support appears in other functions to which the research and development is related, such as the functions that cover energy, health, and defense. In addition, a tax provision, which permits private industry to treat research and development expenses as current costs, rather than as capital investments to be depreciated over a period of years, will provide an estimated $0.7 billion in *tax expenditures* that support research and development in 1977. Special Analysis P, "Federa Research and Development Programs," in the Special Analyses volume of the Budget, discusses the full range of such Federal activities.

Space research and technology.—This category consists entirely of NASA funds for manned space flight; space science, applications, and technology; and supporting space activities.

Outlays for space research and technology in 1977 are proposed to be $3.3 billion, $149 million greater than in 1976. The increase in 1977 is primarily for the continued development of the space shuttle.

Manned space flight.—Manned space flight activities will be concentrated on development of the space shuttle. The shuttle is a reusable space vehicle which will be the key element of a transportation system that will provide a major advance in U.S. space capabilities beginning in the early 1980's. Outlays for manned space flight will be $1.9 billion in 1977, $130 million higher than in 1976.

During 1976 and 1977 NASA will reach major milestones in the shuttle development program, including the roll-out of the first completed shuttle orbiter in September 1976 and the performance of a series of approach and landing tests beginning in 1977. The first manned orbital flight of the shuttle orbiter is scheduled for 1979.

One of the first payloads to be carried into orbit by the space shuttle will be a space laboratory, which is being developed cooperatively with the European Space Agency.

In keeping with the Administration's efforts to restrain Federal spending, procurement of a third shuttle orbiter is being deferred for consideration in 1978. Certain shuttle-related payload and support activities will also be reduced or delayed in order to restrain outlays in the 1977 budget.

Space science, applications, and technology.—The 1977 budget provides funds to continue exploration of the solar system and the universe using automated spacecraft. Outlays for space science, applications, and technology will be $1.1 billion in 1977.

The Pioneer 10 and 11 spacecraft launched in 1972 and 1973 sent back pictures of Jupiter in 1974 and are continuing their exploration of the outer planets. Pioneer 10 is leaving the solar system and Pioneer 11 will fly past Saturn in 1979. Two unmanned Viking spacecraft launched in the late summer of 1975 are en route to Mars and will begin to search for life on the surface of that planet in July 1976. Work is going forward on spacecraft that will explore the atmosphere of Venus in 1978 and fly past Jupiter and Saturn by 1979.

In addition to projects to explore the planets, satellites are being developed to conduct astronomy from Earth orbit. Development will continue in 1977 of high energy and ultraviolet astronomy observatories that will orbit Earth to study the composition of the galaxy and distant parts of the universe. Development of a satellite to be launched in 1979 to study the next peak of solar flare activity will be initiated in 1977.

In the applications program, a third Earth resources technology satellite (LANDSAT) is being developed to gather information from space for agricultural forecasting, geological surveys, and other applications. Also the first of a series of new satellites to provide major improvements in weather forecasting will be launched in 1978. Work

is continuing on a satellite to be launched in 1978 to locate and map potential geothermal sources of energy. A satellite to monitor the Earth's pollution is being prepared for a 1978 launch, and another will monitor ocean conditions and provide improvements in weather prediction and oceanography. In 1977 development will start on a new satellite to be launched in 1980 that will improve mapping of the Earth's magnetic field.

As part of efforts to restrain Federal spending, several new satellites previously planned to be initiated in 1977 will be postponed for consideration in 1978.

Supporting space activities.—Funds are included under this heading to provide tracking and data support to the existing flight programs and to encourage broader utilization of space technology for commercial uses. Outlays for supporting space activities will be $349 million in 1977.

General science and basic research.—The 1977 budget includes funds to assist in providing balanced Federal support of basic research in all scientific disciplines. Outlays for general science and basic research will be $1.2 billion in 1977.

National Science Foundation.—Proposed budget authority for the National Science Foundation will increase by $87 million to $805 million from 1976 to 1977. There will be an increase of almost 20% in obligations for the conduct of basic research, from $523 million to $625 million. This increase will apply to all fields of basic research, but particularly to the physical and life sciences. Growth will continue in international scientific programs such as the international decade of ocean exploration and in national programs such as the climate dynamics program. Funds for the U.S. Antarctic research program will also increase due primarily to additional costs of logistic support. The program of research applied to national needs (RANN) will continue with a focus on environment, productivity, and natural resources.

Energy Research and Development Administration.—Funds for high energy physics research will increase to provide for the construction of a large positron-electron colliding beam facility at the Stanford Linear Accelerator Center. The use of this facility can lead to a new depth of understanding of elementary particles and the fundamental laws of physics. The 1977 budget also provides for the continued utilization of four existing national accelerator facilities. These high-energy facilities assist scientists in advancing knowledge of the basic nature of matter. In addition, increases are included for a balanced program in basic energy sciences to support the future development of both nuclear and non-nuclear energy technologies.

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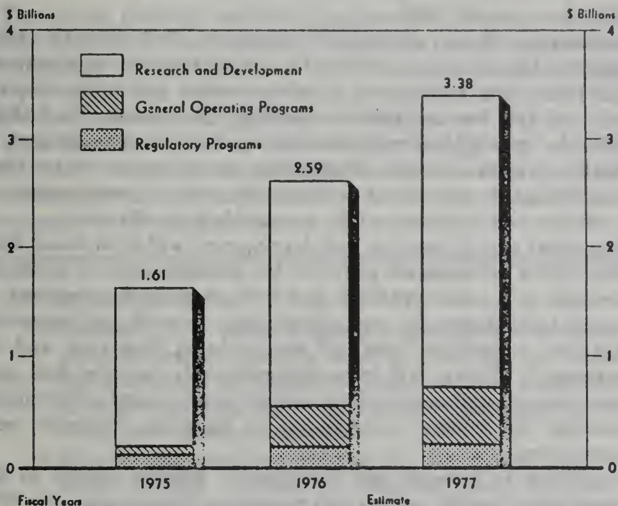
NATURAL RESOURCES, ENVIRONMENT, AND ENERGY

Natural resources, environment, and energy programs are concerned with both present and future needs. They promote the management of the Nation's natural resources, recognizing development, conservation, and environmental objectives that sometimes conflict. Outlays for this function are estimated to be $13.8 billion in 1977 and $14.4 billion in 1978. EXCERPTS: PP. 84, 86, 88-89

Program Highlights

- Establish Energy Independence Authority to provide loans, loan guarantees, and other assistance to selected high priority private sector energy projects.
- Begin initial development of a strategic petroleum storage program to minimize the impact of disruptions in foreign oil supplies.
- Increase outlays for existing and new energy research and development initiatives by 30% in 1977.
- Accelerate the development of technology for the safe long-term management of radioactive wastes from commercial nuclear facilities.
- Increase energy production and encourage energy conservation by the gradual decontrol of oil prices and the immediate decontrol of new domestic natural gas prices.
- Provide $3.8 billion in outlays for the construction of sewage facilities in 1977, a 60% increase over 1976 and a 95% increase over 1975.
- Recommend amendments to the Federal Water Pollution Control Act to focus Federal financial assistance on meeting the needs of existing population and to provide additional incentives for meeting water quality standards in the most efficient manner.
- Prepare for oil and gas leasing planned in frontier areas of the Outer Continental Shelf and promote increased energy development on Federal lands consistent with acceptable environmental standards.
- Provide $300 million for recreation land purchases and development.
- Add 400 personnel to National Park Service staff in 1976 to meet bicentennial needs.

Outlays for Energy[1]



[1] Excludes full effect of Energy Policy and Conservation Act.

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Operating programs promote the development of domestic energy resources and encourage energy conservation. Outlays for these programs will total $478 million in 1977. However, this estimate does not include the full effect of the recently signed Energy Policy and Conservation Act. The allowance for contingencies for fiscal years 1976 and 1977 covers amounts that may be necessary for programs authorized by this Act.

The budget assumes that Congress will approve the proposed Nuclear Fuel Assurance Act, under which ERDA will assist private industry to finance, construct, and operate all future uranium enrichment facilities to meet projected fuel requirements for civilian nuclear power plants.

A strategic petroleum reserve will be developed in order to minimize the impact of disruptions in foreign oil supplies. Energy conservation programs are intended to increase the energy efficiency of new automobiles and many new appliances and to set goals for saving energy in the leading energy-consuming industries.

The budget continues the acceleration of ***energy research and development.*** Outlays for this purpose, under this subfunction, are expected to total about $2.7 billion in 1977, a 30% increase over 1976 and an 85% increase over 1975.

Nuclear research and development outlays in 1977 will total about $1.4 billion. Increases are provided for the construction of an experimental fusion test reactor and a demonstration power plant using liquid metal fast breeder reactor technology. In addition, the budget provides for greatly increased research on the safe management of radioactive wastes and the safeguarding of nuclear materials from theft, which are important to assuring that nuclear power remains a safe, reliable, and environmentally acceptable form of energy.

Nonnuclear energy research and development will total about $900 million. Major increases are provided for demonstration of advanced technologies to produce synthetic fuel from coal, the development of advanced technologies for coal combustion, research on improving gas turbines in order to burn fuel gas produced from coal, and oil shale research. Outlays will increase significantly for solar and geothermal research and development and for development of advanced technologies for energy conservation in buildings, industry, and transportation.

In addition to research and development directly related to the development of specific nuclear and nonnuclear technologies, the 1977 budget includes about $450 million for supporting research. Such research in environmental effects and basic energy sciences is applicable to many different technological objectives. It will involve studies on the effects and control of various pollutants and investigations of the fundamental properties of materials applicable to advanced energy technologies.

. . . (pp. 88-89)

EXCERPTS: PP.126,132 **HEALTH**

The health function includes programs that finance and provide health services (primarily for the aged and poor), support health research, pay for the training of health care personnel, and support the prevention and control of health problems. Outlays for Federal health programs are estimated at $34. 4 billion in 1977, an increase of $2. 3 billion or 7% over 1976. Outlays in 1978 are expected to reach $37. 7 billion, primarily reflecting increases in the medicare program and an additional $500 million for the proposed new Financial Assistance for Health Care Act.

Program Highlights

- Initiate a $10 billion program consolidating 16 health grant programs, including medicaid, through the Financial Assistance for Health Care Act, so that States will have greater flexibility in meeting the health care needs of the low-income population.
- Provide catastrophic protection for the elderly and disabled through medicare by limiting an individual's payments to $500 per year for hospital and nursing home care and $250 annually for doctors' fees.
- Slow health cost inflation by limiting medicare reimbursements for health care services and requiring States to undertake health planning and cost control activities under the Financial Assistance for Health Care Act.
- Reform medicare cost sharing to provide needed program funding and to help assure that hospitalization and medical services are medically necessary.

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Health research and education.—Programs for health research and education include support for research, as well as training and education of health care personnel.

Health research.—Outlays for research will be $2.2 billion in 1977. Current levels of effort will be maintained in major research areas such as cancer and heart disease. Support for emerging research fields—such as immunology, aging, and the effects of the environment upon health—will grow. (p.132)

GENERAL GOVERNMENT

General government programs encompass many fundamental Federal activities including the legislative branch, the Executive Office of the President, collection of revenues and Government-wide operations affecting property, supplies, and personnel. Outlays for general government programs will decrease by $114 million in 1977 to an estimated $3.4 billion. In 1978, outlays for these programs are estimated to be $3.9 billion.

Program Highlights

- Plan for an Office of Science and Technology Policy to give scientific and technological advice and assistance to the President.
- Accomplish Internal Revenue Service functions with reduced staff through anticipated productivity increases and other management improvements.
- Plan for proposed new territory, the Commonwealth of the Northern Mariana Islands.
- Convene a National Women's Conference.

Legislative functions.—By law, the President's budget contains estimates for the legislative branch as they are submitted by that branch. The legislative branch proposes to spend $789 million in 1977 for the Congress, the General Accounting Office, the Congressional Budget Office, and other activities in this subfunction.

Executive direction and management.—Outlays for the White House, the Executive Office of the President, and related activities are expected to be $75 million.

The Office of Science and Technology Policy is planned to begin operations in 1976, subject to the enactment of authorizing legislation now before the Congress. The Office will advise the President on scientific and technological aspects of national policies, programs, and issues, and on the use of new discoveries in science and technology in addressing national problems.

B. <u>The Government-Wide Federal Research and Development Program.</u>

<u>Special Analysis P.</u> As noted above, the only place in the Budget where the Government-wide research and development effort is summarized is in the annual special analysis section on Federal Research and Development Programs. In Fiscal Year 1977, this section is Special Analysis P. The full text of Special Analysis P is printed below.

<u>Special Analysis K.</u> Health research and research facilities outlays are expected to reach the $3 billion mark for Fiscal Year 1977. There is relatively little explanatory detail on this area, either in the functional program description or in Special Analysis P. For this reason, excerpts from Special Analysis K, Federal Health Programs, which focus on health research are included here.

<u>Federal Council Report on the Federal R & D Program, FY 1976, not continued.</u> Last year at the same time the Budget was presented, the President's Science Adviser made public a Report on the Federal R & D Program, FY 1976, prepared by the Federal Council for Science and Technology from information supplied by the departments and agencies and assistance from the Office of Management and Budget. This study constituted what was in effect an enlarged and expanded special analysis of the Federal R & D program, by agency, and by major programs. Despite admitted weaknesses, the report was favorably received and widely used. Although there was support in certain quarters for the continuation of this report on an annual basis, the effort was not repeated for the Fiscal Year 1977.

The President's Science Adviser's Report on the Federal R & D Program-FY 1977. Dr. H. Guyford Stever, Director, National Science Foundation, and Science Adviser to the President, presented highlights of the FY 1977 Federal R&D budget on January 21, 1976. The press release on this briefing included several exhibits in the form of tables and charts, which for the most part, are supplementary to exhibits presented in Special Analysis P. This press release is reproduced below. It constitutes a fitting conclusion for this report by Congressional Research Service on the Federal research and development program for Fiscal Year 1977.

NATIONAL SCIENCE FOUNDATION

OFFICE OF THE SCIENCE ADVISOR NEWS

Jack Renirie
(202) 632-5728
Home: (703) 978-9104

FOR RELEASE:
STRICTLY EMBARGOED
NOT FOR RELEASE BEFORE
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76-SAI

REPORT ON THE FEDERAL R&D PROGRAM - FY 1977

Federal obligations for research and development for Fiscal Year 1977 will total $24.7 billion, of which $23.5 billion are for the conduct of R&D with the balance designated for facilities. This represents an 11 percent increase over the FY 1976 level (Chart 1). This rise is significant, especially in view of the tightness of this years overall U.S. budget, and that illustrates the high priority placed by the Administration, on science and technology because of what it can contribute to the achievement of national goals.

Three-fourths of the $2.2 billion increase in the amount budgeted for obligations for R&D conduct in FY 1977 will support development programs which will show a 11 percent increase over FY 1976 (Chart 3) while total research will show a 9 percent rise.

Of particular note is the fact that within the total research budget, basic research will increase 11 percent over FY 1976 to $2.6 billion with the largest agency increases shown in the basic budgets of NSF (20 percent),

DoD (16 percent) and HEW (12 percent) (Chart 4). The NSF increase reflects an awareness of the importance of this agency's role in the support of basic research and the Administration's recognition of its importance to the Nation. In FY 1977, 77 percent of the NSF total budget is for basic research. This Federal effort accounts for about 70% of the National activity in basic research and this Federal support is justified because the benefits of basic research generally accrue to all of society.

The FY 1977 budget provides for an increase in obligations for the conduct of the defense research and development effort, including nuclear weapons programs of ERDA, totaling $1.4 billion, up 13 percent over the FY 1976

budget (Chart 1). The defense R&D effort will include the continuation of a number of major programs for the improvement of ballistics missile systems and modernization of tactical forces plus the initiation of programs for air-launched and sea-launched long-range cruise missiles.

Space R&D programs will remain level in FY 1977 at $2.9 billion, to continue scheduled development of the Space Shuttle and unmanned spacecraft and to start work on a new satellite for the Solar Maximum mission. Due to fiscal constraints, it was necessary to defer the Space Telescope for consideration in FY 1978. In this year's Special Analysis "P", major satellites and launch vehicles are classified as development, instead as conduct of basic and applied research.
In FY 1977 civilian R&D funding other than space will increase $0.8 billion, 10 percent over FY 1976. The share of total Federal R&D obligations for civilian research and development in FY 1977 will amount to 37 percent; a decade ago the civilian programs accounted for 20 percent. Among the major civilian R&D programs, the FY 1977 budget places highest priority on the development of energy technologies (Chart 5, 6) with the increases occurring in both the conduct of nuclear research and development and in nonnuclear programs, especially for geothermal, conservation, and solar programs. A number of areas related to the Federal health R&D programs will receive increases in FY 1977, particularly research into human biological processes. Cancer Research and Heart and Pulmonary disease research will increase slightly over previous levels. Total R&D obligations for NIH will increase by $181 million in FY 1977 to approximately $2 billion.

Recognizing the importance of food to the welfare of the U.S. and the world, a $20 million increase in the Department of Agriculture's basic research funding will be directed toward areas that may lead to increased food production (bringing the total for basic research in this area to $197 million for FY 1977).

The FY 1977 budget in terms of agency obligations for this conduct of R&D is shown in Charts 7 and 8. They show the relatively large increases in ERDA (17%), NSF (16%), and DoD ((13%). There are a few areas of reduced R&D funding.

Federal research and development facilities obligations will increase by 34 percent to $1,215 million in FY 1977 from $909 million in FY 1976. While a number of agencies have R&D facilities projects planned for FY 1977, the major programs will be: DoD's $180 million increase planned for the Aeropropulsion System Test Facility to be built at the Arnold Engineering Development Center; ERDA start of construction of a $78 million major new positron-electron colliding beam facility for high energy physics at the Stanford Linear Accelerator Center which is budgeted in FY 1977 for $6 million for design and initial construction; and NASA funds to initiate construction of a new wind tunnel at the Langley Research Center. An increase of $30 million in ERDA is due to the initiation of design and construction of 3 commercial scale plants to demonstrate the conversion of coal to industrial fuel gas, pipeline-quality gas, and clean liquid fuel.

The FY 1977 budget provides $2.6 billion for research and development at the Nation's universities and colleges, up 9 percent over FY 1976 (Charts 9,10). One-half of these funds, $1.3 billion will come from HEW programs. The National Science Foundation's budget reflects a rise in academic R&D obligations in FY 1977 of $85 million to $550 million, the largest increase planned by any agency.

The following are some highlights in the FY 1977 Federal R&D budget.

Energy

The FY 1977 budget provides over $2.6 billion in authorities for direct energy R&D - about a 38% increase over last year's total of $1.9 billion. Of this total $1.6 billion will be for nuclear and 1.0for non-nuclear R&D. In addition there will be $0.6 billion provided for supporting R&D in basic research and environmental health effects (Chart 6). The Energy Research and Development Administration (ERDA) accounts for approximately $2.3 billion or 90% of the total budget authority for direct energy R&D. ERDA was given a legislative mandate to prepare and annually update an energy R&D "plan" that covers Federal energy research development and demonstration. This report was issued in July of 1975 and is currently being updated so as to be available soon. The plan includes R&D priorities among various energy technologies. The report will cover ERDA activities and the other direct and supporting energy R&D which is performed by the Nuclear Regulatory Commission, Department of the Interior, National Science Foundation and the Environmental Protection Agency.

Defense

The Department of Defense obligations for conduct of R&D will increase by about $1.3 billion over the FY 1976 level reaching a total of $11.2 billion. Dollarwise, the majority of the dollar increase is in development although nearly $280 million is added in the research category. However, percentagewise, research obligations show the greater percentage increase. DoD support for university research is $225 million which represents approximately an 11% increase over FY 1976. The total basic research activity undertaken or sponsored by DoD will increase from $330 million in FY 1976 to $383 million in FY 1977. Much of this increase will be performed by contractors rather than DoD itself.

In development, both strategic and tactical systems will receive increased support. As the Trident and B-1 bomber programs go from the development to the procurement stage, development dollars will be increasingly devoted to ballistic missile warhead improvements, cruise missiles, and modernization of the tactical force.

The DoD portion of the Federal R&D budget for FY 1977 will remain at about the same fraction of the total R&D program as it is in FY 1976, which is about 50 percent.

ERDA military-related R&D programs will increase from $748 million in FY 1976 to $775 million in FY 1977. These activities include both naval reactor development and nuclear weapons development and testing.

National Science Foundation

The obligation authority for R&D exclusive of research facilities of the National Science Foundation is expected to increase 16% from $628 million in FY 1976 to $726 million in FY 1977. The fact that the increase for NSF is higher than the average increase across all Federal Departments and Agencies reflects the importance that the Administration attaches to basic research, and the fact that a very large proportion of the NSF budget is devoted to basic research. In FY 1975 and FY 1976, NSF allocated 83% of its total R&D funds to basic research. There will be a slight increase in this percentage in FY 1977 bringing it to 86%. There is a corresponding reduction taken mainly from the applied research activities, principally those conducted in the Research Applied to National Needs (RANN) Program. The decreases in the RANN budget are attributable to shifting of responsibility for fire research to the Commerce Department and to shifting responsibility of more of the energy-related research to ERDA.

NSF traditionally spends about three-fourths of its R&D at colleges and universities. Thus the increase in NSF's budget coupled with the fact that most of the NSF funded basic research is conducted at colleges and universities account for an anticipated 18% increase in NSF research support activities in colleges and universities.

Civilian Space R&D

The NASA obligation authority for space R&D will increase from approximately $3.2 billion in FY 1976 to $3.3 billion in FY 1977. Currently, the U.S. space program relies on expendable rockets to launch earth-orbiting and planetary spacecraft. The Space Shuttle, which will be operational in 1980, will for the first time utilize a reusable space vehicle and will provide the capability for a wide variety of missions, including the recovery of satellites for repair and reuse. During 1977 NASA will conduct the first approach and landing tests of the orbiter, aimed at the first manned orbital flight in the mid-1970's.

Space science programs will emphasize the exploration of the solar system and the universe using unmanned spacecraft. The continuing planetary and astronomy programs are described in detail in the Special Analysis "P". A Solar Maximum Mission will be initiated as a new start in FY 1977 for launch in 1979 to study the sun during the next period of peak solar flare activity in 1979-80.

There are also continuing space applications programs which are listed in the Special Analysis "P". A satellite to be launched in 1980 to map the

Earth's magnetic field will be initiated in FY 1977.

The FY 1977 NASA budget reflects several major deferrals which include a one-year delay in initiation of production of the third Shuttle Orbiter, postponement for consideration in the FY 1978 budget of starting development of the Space Telescope; and deferral of consideration of a Pioneer-type orbiter and probe of Jupiter.

Aeronautical R&D

The civilian aeronautical research and technology program at NASA will increase approximately 13 percent to $364 million in FY 1977. It will continue to emphasize the reduction of engine noise in existing as well as future aircraft and the development of clean, quiet, efficient propulsion systems in an effort to mitigate undesirable environmental effects of civil and military aircraft. A major thrust of the 1977 aeronautical research and technology program is focused on a variety of efforts leading toward improving aircraft performance in order to reduce energy requirements. Areas of emphasis include improved materials and composites, aircraft structures, and propulsion systems. The 1977 program also includes the initial increment ($25M) for the constuctior of a National Transonic Facility, a much needed tool for research in aerodynamic design for the transonic regime.

The Department of Defense budget for FY 1977 includes initial funds for the constuction at the Arnold Engineering Development Center in Tullahoma, Tennessee of an Aeropropulsion Systems Test Facility. This aircraft engine test complex will be particularly valuable as a means of minimizing costly testing and engineering modifications in the final stages of aircraft development and certification.

The 1977 budget reflects deferral of consideration of modification and repowering of the 40' x 80' wind tunnel at the NASA Ames Research Center.

6

Health, Education, and Welfare

The Department of Health, Education, and Welfare (HEW) obligations in 1977 for the conduct of R&D will increase by $201 million over the 1976 level, reaching a total of $2,570 million. Obligations for R&D facilities will be $11 million.

The National Institute of Health (NIH) basic research emphasis will be given to studies on the immunology of cancer, arthritis, and various neurologic diseases. Other fundamental studies will concentrate on the cellular and molecular basis of diseases to help understanding and treatment of such diseases as arthritis, diabetes, genetic and neurologic disorders and allergies. Development of vaccines against hepatitis and influenza will continue and the specialized centers for heart and lung research will be expanded. In the new National Institute on Aging, increased attention will be given all aspects of the aging process -- physical, biological and behavioral.

It is planned for NIH to increase R&D obligations by $181 million, from $1.797 million in 1976 to $1.978 million in 1977. Budget decreases in Mental Health R&D will be decreased to $83.0 million). Research will be expanded into the causes of mental illness in such areas as schizophrenia and depression.

The Social R&D Policy Research of the Department will continue assessment of the impact of current or proposed social policies and programs on the poverty population with a budget of $25 million in 1977, level with 1976. In addition, it provides numerous opportunities for addressing policy issues not covered elsewhere in the government.

The major emphasis of income maintenance and employment policy research is to obtain reliable results on the behavioral and societal effects of alternative income maintenance policies, with emphasis on the labor supply or work incentive effects of such policies.

Policy Research in the health area is devoted primarily to support of the Health Insurance Study, which is designed to measure the impact of a wide range of cost-sharing arrangements on the demand for health care and the effects on status over time.

U.S. Department of Agriculture

Obligations of the Department of Agriculture for the conduct of research and development, excluding construction of facilities, will increase from $483 million in 1976 to $507 million in 1977.

New funds will be provided in 1977 for emphasis on basic research in such areas as cell biology, improvements in the photosynthesis process, and new research on nitrogen fixation; increased efficiency in the production of meat animals; developing additional sources of usable proteins from vegetable sources; and protecting against devastating losses to major food crops resulting from genetic vulnerability to disease by collecting, testing and preserving diverse germplasmic materials.

Environmental research will include the further development of non-chemical means of controlling agricultural pests, and the development of information required for the clearance of agricultural pesticides for use in cooperation with the Environmental Protection Agency.

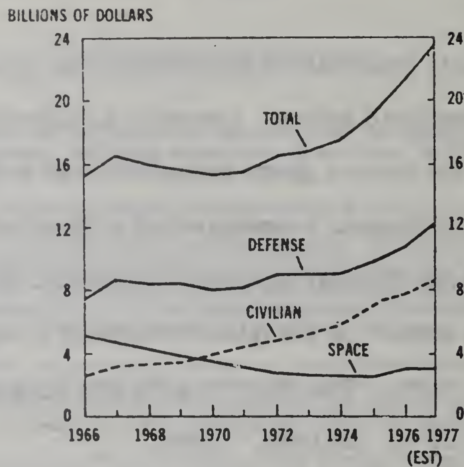
The Forest Service Research program will continue research on critical forestry problems including habitat needs for rare and endangered species, additional accelerated forest pest control, practices to speed up timber and range production, research on non-point source pollution, and improved rehabilitation techniques for surface-mined disturbed lands.

With 1977 budget increases, the Economic Research Service will give added attention to: improving its forecasting and information systems, developing estimates of farm production costs, initiating work to better estimate marketing costs and margins, and appraising developments in foreign markets for U.S. farm products.

[Editor's note:

The 10 charts which are referenced in the preceding report are reproduced below. Attention is called to the fact that while the source given for each chart is Special Analysis P, 1977 Budget, a comparison of these charts with exhibits in the Special Analysis shows that only charts 2 and 10 appear in the latter document in essentially the same form. The other charts are supplementary, not duplicative, exhibits. DMB]

FEDERAL OBLIGATIONS FOR CONDUCT OF
R&D BY MAJOR PROGRAM AREA
FY 1966-77



SOURCE: SPECIAL ANALYSIS P, 1977 BUDGET

STIA 76-1400
1-20-76

Chart 1

CONDUCT OF FEDERAL R&D BY MAJOR PROGRAM AREA
(BILLIONS OF DOLLARS)

	FY1975 ACTUAL	FY 1976 EST.	FY 1977 EST.	DIFFERENCE FY 1976-77	% CHANGE FY 1976-77
TOTAL	$19.0	$21.3	$23.5	+$2.2	+11%
DEFENSE, INCLUDING ERDA MILITARY–RELATED PROGRAMS	9.6	10.6	12.0	+1.4	+13%
SPACE PROGRAMS	2.5	2.9	2.9	-0-	-0-
CIVILIAN PROGRAMS	6.9	7.8	8.6	+.8	+10%

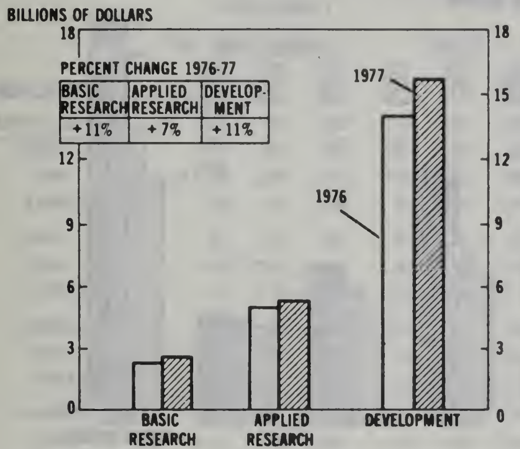
SOURCE: SPECIAL ANALYSIS P, 1977 BUDGET

STIA 76-1399
1-20-76

Chart 2

CRS - 47

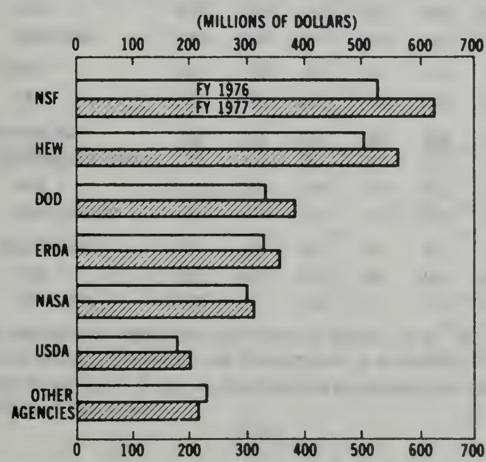
FEDERAL OBLIGATIONS FOR CONDUCT OF BASIC RESEARCH, APPLIED RESEARCH AND DEVELOPMENT, FY 1976 AND 1977 (ESTIMATES)



SOURCE: SPECIAL ANALYSIS P, 1977 BUDGET

Chart 3

OBLIGATIONS OF MAJOR AGENCIES FOR CONDUCT OF BASIC RESEARCH, FY 1976 AND 1977 (ESTIMATES)

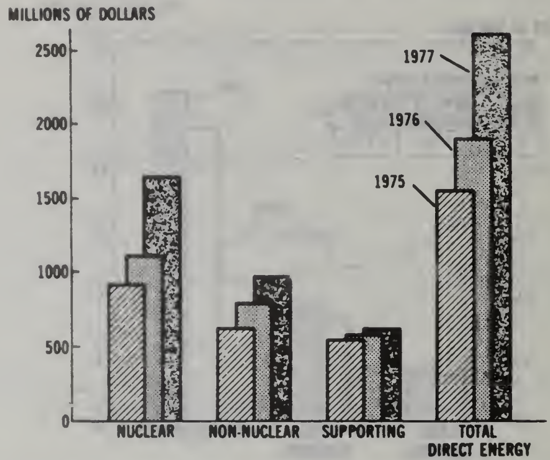


SOURCE: SPECIAL ANALYSIS P, 1977 BUDGET

Chart 4

PRELIMINARY ANALYSIS OF FEDERAL DIRECT ENERGY R&D ACTIVITIES FOR FY 1977

(BUDGET AUTHORITY)



SOURCE: SPECIAL ANALYSIS P, 1977 BUDGET

STIA 76-1406
1-20-76

Chart 5

PRELIMINARY ANALYSIS OF FEDERAL ENERGY RESEARCH & DEVELOPMENT ACTIVITIES FOR 1977

($MILLIONS)

	BUDGET AUTHORITY			OUTLAYS		
	1975	1976	1977	1975	1976	1977
DIRECT ENERGY R&D:	1544	1897	2610	1132	1659	2239
NON-NUCLEAR R&D:	626	791	968	276	685	874
FOSSIL	393	468	542	162	389	508
ERDA	(335)	(398)	(477)	(138)	(333)	(442)
* OTHER (DOI,NSF)	(58)	(70)	(65)	(24)	(56)	(66)
SOLAR	53	120	160	31	88	119
ERDA	(42)	(115)	(160)	(15)	(86)	(116)
OTHER (NSF)	(11)	(5)	(-)	(16)	(2)	(3)
GEOTHERMAL	34	34	50	25	33	47
ERDA	(28)	(31)	(50)	(21)	(32)	(46)
OTHER (NSF)	(6)	(3)	(-)	(4)	(1)	(1)
CONSERVATION	53	82	120	30	63	94
ERDA	(36)	(75)	120	(21)	(56)	(91)
OTHER (NSF, NASA)	(17)	(7)	(-)	(9)	(7)	(3)
ENVIRONMENTAL CONTROL	93	87	96	28	112	106
ERDA	(8)	(13)	(16)	(7)	(12)	(15)
OTHER (EPA,NRC)	(85)	(74)	(80)	(21)	(100)	(91)
NUCLEAR R&D:	918	1106	1642	856	974	1365
FISSION	735	856	1250	705	750	1061
ERDA	(678)	(757)	(1133)	(651)	(669)	(961)
OTHER (NRC)	(57)	(99)	(117)	(54)	(81)	(100)
FUSION	183	250	392	151	224	304
ERDA	(183)	(250)	(392)	(151)	(224)	(304)
SUPPORTING R&D:	549	584	624	405	506	589
ENVIRONMENTAL EFFECTS	239	250	257	162	237	263
ERDA	(171)	(193)	(203)	(148)	(185)	(199)
OTHER (EPA,NSF)	(68)	(57)	(54)	(14)	(52)	(64)
BASIC RESEARCH	310	334	367	243	269	326
ERDA	(191)	(211)	(227)	(165)	(188)	(204)
OTHER (NSF)	(119)	(123)	(140)	(78)	(81)	(122)

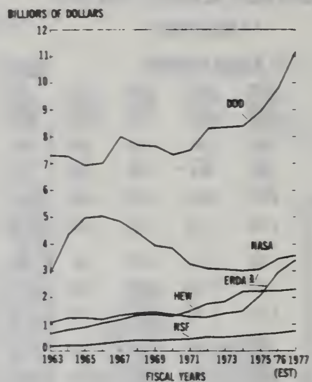
A FINAL COMPREHENSIVE AND DETAILED ANALYSIS WILL BE AVAILABLE IN THE "NATIONAL PLAN FOR ENERGY RESEARCH, DEVELOPMENT, AND DEMONSTRATION" TO BE PUBLISHED BY ERDA.

* LISTED BESIDE "OTHER" IN THIS AND FOLLOWING LINES ARE AGENCIES WITH MAJOR R&D ACTIVITIES IN EACH CATEGORY.

STIA 76-1406 1-29-76

Chart 6

FEDERAL OBLIGATIONS FOR CONDUCT OF RESEARCH AND DEVELOPMENT BY SELECTED AGENCIES FY 1963-77



a/ AEC DATA PRIOR TO 1974

SOURCES: NSF, FEDERAL FUNDS SERIES, FY 1963-74; SPECIAL ANALYSIS P, 1977 BUDGET, FY 1975-77

STIA 76-1397
1-20-76

Chart 7

FEDERAL OBLIGATIONS FOR CONDUCT OF R&D BY SELECTED AGENCIES (MILLIONS OF DOLLARS)

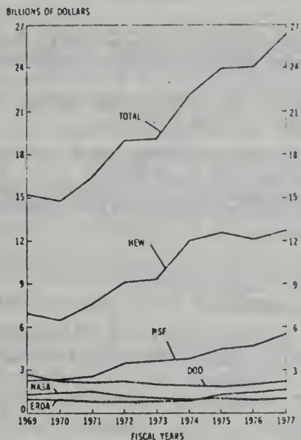
DEPARTMENT OR AGENCY	FY 1975 ACTUAL	FY 1976 EST.	FY 1977 EST.	DIFFERENCE FY 1976-77	% CHANGE FY 1976-77
TOTAL	$19,023	$21,338	$23,465	+ $2,127	+ 10%
DEPARTMENT OF DEFENSE	8,987	9,879	11,198	+ 1,319	+ 13%
NASA	3,088	3,473	3,573	+ 100	+ 3%
ERDA	2,071	2,812	3,282	+ 470	+ 17%
HEW	2,395	2,369	2,570	+ 201	+ 8%
NATIONAL SCIENCE FOUNDATION	604	628	726	+ 98	+ 16%
DEPARTMENT OF AGRICULTURE	424	483	507	+ 24	+ 5%
DEPARTMENT OF TRANSPORTATION	291	340	319	– 21	– 6%
DEPARTMENT OF INTERIOR	296	332	316	– 16	– 5%
EPA	258	305	241	– 64	– 21%
DEPARTMENT OF COMMERCE	222	247	243	– 4	– 2%
VETERANS ADMINISTRATION	99	108	106	– 2	– 2%
NUCLEAR REG. COMMISSION	61	97	109	+ 12	+ 12%
HUD	57	62	70	+ 8	+ 13%
DEPARTMENT OF JUSTICE	44	65	41	– 24	– 37%
ALL OTHER	126	138	164	+ 26	+ 19%

SOURCE: SPECIAL ANALYSIS P, 1977 BUDGET

STIA 76-1400
1-20-76

Chart 8

FEDERAL R&D OBLIGATIONS TO COLLEGES AND UNIVERSITIES
FY 1969-77



a/AEC DATA PRIOR TO 1974
SOURCES: NSF, FEDERAL FUNDS SERIES, FY 1963-74; SPECIAL ANALYSIS P, 1977 BUDGET, FY 1975-77

Chart 9

FEDERAL R&D SUPPORT TO COLLEGES AND UNIVERSITIES
(MILLIONS OF DOLLARS)

DEPARTMENT OR AGENCY	OBLIGATIONS FY 1975 ACTUAL	FY 1976 EST.	FY 1977 EST.	DIFFERENCE FY 1976-77	CHANGE FY 1976-77
TOTAL	$2,399	$2,407	$2,635	+$228	+9%
DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE	1,269	1,218	1,302	+84	+7%
NATIONAL SCIENCE FOUNDATION	447	465	550	+85	+18%
DEPARTMENT OF DEFENSE-MILITARY	190	202	225	+23	+11%
ENERGY RESEARCH & DEVELOPMENT ADMINISTRATION	135	141	166	+25	+18%
DEPARTMENT OF AGRICULTURE	108	120	128	+8	+7%
NATIONAL AERONAUTICS & SPACE ADMINISTRATION	108	107	107	-	-
ALL OTHERS	142	154	157	+3	+2%

SOURCE: SPECIAL ANALYSIS P, 1977 BUDGET

Chart 10

FEDERAL RESEARCH AND DEVELOPMENT IN THE FISCAL YEAR 1977 BUDGET RECENT SELECTED REFERENCES

Battelle forecasts an 11.1 percent increase in R&D funding during 1976 in U.S. Press release from Battelle Columbus Laboratories, December 19, 1975. 5 p. Typescript

Press release on the Fisher annual Battelle forecast for R&D expenditures. (see Fisher citation below.)

Carey, William D. National priorities: Round one to basic science. Editorial. Science, v. 191, February 6, 1976: 427.

Urges cautious optimism concerning the Federal R&D budget. Initiation of a special Report on the Federal R&D Budget for 1977 by AAAS, to be issued in late spring 1976, is announced.

Federal budget digest: Defense/energy/research budgets increase. Science trends, v. 35, January 19-26, 1976: 97-108.

This is a special report on the fiscal 1977 R&D budget.

Fisher, W. Halder. Probable levels of R&D expenditures in 1976; forecast and analysis. Columbus, Battelle Memorial Institute. December 1975. 9 p.

This report for calendar 1976 is based upon a continuing study of U.S. expenditures for research and development and considers both public and private sources.

Ford budget emphasizes basic research. Chemical and engineering news, v. 54, Jan. 26, 1976: 4.

R&D expenditures in U.S. to rise about 11% in 1976. Chemical and engineering news, v. 54, Jan. 5, 1976: 4.

On the annual R&D forecast for 1976 by W. Halder Fisher, Battelle Columbus Laboratories.

Walsh, John. 1977 budget: Rise in R&D funds includes boost for basic research. Science, v. 191, February 6, 1976: 444-446.

This is the annual review of the budget from Science.

"Seventy Issues: Fiscal Year 1977 Budget"

[From the Executive Office of the President, Office of Management and Budget, January 21, 1976, p. 32]

PREFACE

Literally thousands of Presidential decisions are reflected in the Budget each year. Those already fully familar with and very interested in a given program will want to carefully review the relevant portions of all four basic budget documents—The Budget in Brief, the Budget, the Special Analyses and the Appendix—and also the budget materials issued by the particular department or agency.

To a large extent the budget documents are written for the interested expert; and the Budget descriptions of programs, existing and proposed, are highly condensed.

The collection of materials that follows will be, by and large, too general to be useful to the interested expert. It was developed by the staff of the Office of Management and Budget and the Domestic Council, with the assistance of the departments and agencies, with the idea that each year there are a number of issues which will be of more general interest than the rest. It is our purpose to provide somewhat more information than is in the Budget in Brief, or Budget, and either satisfy the general reader's needs or stimulate enough interest to encourage him or her to pursue the subject in more depth.

As with any first effort, there will be shortcomings; certain issues included will turn out to be of little interest; some issues of at least equal interest to those covered will be identified; in some papers the problem will be too little information; in other too much; in some, questions will arise as to whether the treatment given was balanced; in all, restraints upon preparation time limited the attention all would like to have given to improvements in presentation. However, we believe that the effort to be helpful is worth these probable deficiencies.

1977 BUDGET, FEDERAL RESEARCH AND DEVELOPMENT

	Obligations 1976	1977 (millions)	Percent change 1976–77
Conduct of R. & D.:			
Defense	9,879	$11,198	+13
NASA	3,473	3,573	+3
ERDA	2,812	3,282	+17
HEW	2,369	2,570	+8
NSF	628	726	+16
Agriculture	483	507	+5
Other	1,694	1,609	−5
Total, conduct of R. & D	21,338	23,465	+10
R. & D. facilities	909	1,215	+34
Total, all R. & D	22,247	24,680	+11
Included in total funds for the conduct of R. & D. are Federal support for both:			
Basic research (in Government labs, universities, and industry)	2,330	2,640	+11
Grants and contracts to universities for both basic and applied (e.g., medical) research	2,407	2,635	+9

Overall strategy for R. & D.

The Government invests in R&D largely to develop new products or processes. It is not a separately budgeted activity of the Federal Government and should not be viewed as an "end-in-itself." Rather it is a means whereby agency or national goals can be achieved more effectively or more efficiently; R&D on new weapons systems can improve the Nation's defense; R&D on the space shuttle can improve our ability to work in outer space; R&D on the liquefaction of coal can improve our ability to use coal rather than rely on imported oil, R&D on new varieties of wheat can increase argicultural production to meet domestic needs and sales to foreign countries.

Federal R&D covers a wide variety of activities. In varying degrees agency R&D programs include: Basic research (to develop fundamental new knowledge); Applied research (to apply new knowledge to specific applications); Development (to do the engineering for new devices or systems); Demonstration (to build experimental new devices or systems to see how well they work and what they cost to build and operate at full scale).

Private industry, foundations, universities, and others also invest in R&D. The private sector accounts for nearly half of the national investment in R&D. The Federal Government seeks to encourage—through its R&D programs, as well as through its tax policies—continued and expanded private investment in R&D so that overall national goals may be better met through science and technology.

The Federal Government focuses its R&D investment in three broad areas to meet:

Direct Federal needs, where the Government has full responsibility, such as in space and national defense.

General economic and human welfare needs, where the Federal Government assumes major responsibility because there are not sufficient incentives for the private sector to invest enough to meet national needs, such as in basic research, medicine, environment, and agriculture.

Specific national needs, where Government shares responsibility and uses Federal funds to accelerate and augment the efforts of private sector because of the overriding national interest, and there is a need to provide the Nation with technological options for the future, such as in energy.

Constraints must be exercised and priorities set for R&D not only because of overall budget limitations, but also to:

Avoid overtaking private sector responsibilities to produce, market and utilize technical advances, such as technologies to conserve energy in industrial manufacturing where there is a strong incentive for private industry to do the job.

Encourage cost-sharing with private sector for technologies ready for commercial demonstration, such as oil-shale development.

Avoid investing in technology currently seen to have future questionable benefits, such as: A large space station; Expensive high-speed trackless trains operating on cushions of air.

Budget and program highlights (fiscal year 1977)

In defense:

Funding will increase for basic research, ballistic missile warhead improvements, and providing options for a new intercontinental ballistic missile system.

Funding will decrease in the Trident long-range submarine and missile system because these systems are entering procurement.

In space: Continue development and testing of the space shuttle: Defer procurement of a third space shuttle in recognition of budget constraints.

In other civilian agencies:

In the Energy Research and Development Administration: Increase research and development to assure the availability of safe and reliable nuclear power; continue development of fusion technology and the nuclear breeded demonstration program; expand research to make better and more environmentally acceptable use of fossil fuel through gasifying or liquefying coal; assist industry in advanced energy conservation technology and technologies involving solar and geothermal energy.

In the Department of Health, Education, and Welfare: Augment research on immunology, aging, and environmental health.

In the National Science Foundation: Increase efforts in basic research including emphasis on physics, engineering, chemistry, and materials.

In the Department of Agriculture: Expand research on the basic biological processes underlying agriculture production, and development of additional sources of usable vegetable protein.

In other agencies: Obligations decline in total for the conduct of R&D in those other agencies with less than $5 billion each in R&D. This reflects the impact of a few agencies, such as the Environmental Protection Agency and the Department of Commerce, where some R&D efforts have been completed or postponed, or where there were usually high obligations in 1976.

In facilities: Increase obligations by 34% to provide the necessary plant and equipment for scientists and engineers to conduct their research and development. Key increases are: An aircraft test complex for the Department of Defense; a wind tunnel for the National Aeronautics and Space Administration; a variety of development and demonstration projects for the Energy Research and Development Administration, primarily in fossil, fusion, and fission energy technologies.

In universities and colleges: Increase grants and contracts for basic and applied research in health, energy, and science in general, by 9 percent.

In overall support of *basic research:*

In the National Science Foundation: Increase basic research funding by nearly 20 percent to strengthen its key role in support of such research.

In other agencies: Strengthen basic research in support of their mission.

Provide an *overall* Federal increase of 11 percent for basic research.

In addition, strengthen the competitiveness of U.S. research in high energy physics by starting, in 1977, a new machine (a "colliding beam" facility) to develop and test new theories on the ultimate nature of matter.

FISCAL YEAR 1977 BUDGET, NATIONAL SCIENCE FOUNDATION

	Fiscal year 1976		Fiscal year 1977		
	Budget authority (millions)	Percent of total	Budget authority (millions)	Percent of total	Percent change 1976–77
Basic research (e.g., biology, math, chemistry geology, astronomy)	$523	71	$625	77	+19
Applied research (e.g., earthquake resistant structures)	110	15	90	11	—18
Science education (e.g., fellowships, curriculum improvement)	65	9	65	8	
Other (e.g., international science programs, administrative expenses)	34	5	32	4	—6
Total NSF	732	100	812	100	+11

Basic research: The primary responsibility of NSF

The major responsibility of the National Science Foundation (NSF) is support of basic research.

Basic research may be defined as the intensive study of natural laws and phenomena or human behavior for the purpose of learning or increasing knowledge.

Basic research is generally free from practical needs to meet immediate objectives, but it is regarded as the foundation for future applications of science to improve our national defense, our economy, and our human welfare.

Individual judgments will certainly vary about the worth of particular basic research projects. Nevertheless, some examples help understanding the type and range of activities supported by NSF in fulfilling its responsibility for strengthening basic research:

Discoveries of fundamental particles that make up the nucleus of atoms and molecules (discoveries like this one, in the past, have led to the major advances in chemistry and electronics that we have seen in recent years);

Along with increases in NSF support, *total* Federal support will grow from $2.38 billion in 1976 to $2.64 billion in 1977, an increase of 11%.

Increases for basic research in NSF and other Federal agencies will help reverse a decline that has taken place in the last several years in terms of real spending on basic research. Because inflation has been at a higher rate since 1967 than the Federal increases in funds for basic research, the level of scientific work supported by the Federal Government has decreased.

The program of Research Applied to National Needs (RANN) will be reduced reflecting a shift of responsibilities in applied energy reseach (particularly for solar and geothermal power) from NSF to the Energy Research and Development Administration.

The science education program does not grow because evaluations are being conducted on the need for and effectiveness of some of these programs.

The NSF uses its funds to complement the funding of other agencies (which support basic research to serve their specific mission needs). Thus, NSF aids in assuring that enough basic research is conducted in this country to meet the Nation's need for new fundamental knowledge to support future advancements in technology; that Federal support is "balanced" across the many disciplines of basic science; and that highly innovative ideas, which other agencies cannot support in relation to their mission needs, are pursued.

The importance of this responsibility to NSF is indicated by the budget where 77% of the NSF funding is for basic research in 1977.

NSF finances almost a quarter of the *Federal* effort in basic research. Responsibility for the other three-quarters is divided among several Federal agencies, which fund basic research oriented toward their mission.

These include chiefly the: Department of Defense; National Aeronautics and Space Administration; Energy Research and Development Administration; Department of Health, Education, and Welfare (particularly the National Institutes of Health).

Studies of changes in the Earth's magnetic and volcanic forces (to develop ways to predict the timing and location of earthquakes);

Development of advanced techniques for modeling and analyzing the national economy (which may provide the basis for improved future management of national growth);

Identification of the functioning of chemical and biological catalysts (which are essential for improved chemical industrial processes);

Investigation of the novel properties of metals at extremely low temperatures (which is providing the basis for major technological advances in computing, measurement and other electronic systems).

Significant responsibilities of NSF

The Foundation supports a limited amount of applied research which is focused on important national problems not addressed by other federal agencies. This is chiefly through the RANN program (Research Applied to National Needs) which was, for example, the program that initially supported Government efforts in solar and geothermal R&D (now transferred to ERDA).

NSF as part of its responsibilities also support the U.S. Antarctic Program for scientific research using the logistic support of the Departments of Defense and Transportation. This program which supports important overall U.S. interests in the Antarctic will total $45 million in FY 1977.

Science education programs of NSF provide for both the training of scientists and engineers and the development of new approaches for teaching science.

Budget and program highlights (FY 1977)

Overall budget authority for NSF will increase by 11% from $732 million to $812 million, because of the need to strengthen Federal support of basic research in the national interest.

Budget authority for basic research will increase by 19%, from $523 million to $625 million.

Nearly all fields of basic research will share these increases including programs in biology, chemistry, physics, engineering, math, and astronomy.

These increases are balanced with increases for basic research provided in the 1977 budget in other agencies, particularly the Department of Defense, the Energy Research and Development Administration, the Department of Agriculture, and the Department of Health, Education and Welfare.

The Federal effort in support of basic research is predominant in the Nation because the benefits of basic research generally accrue to all of society.

Federal efforts account for about 70% of the national activity in basic research. Other support for basic research comes from non-profit organizations, private industry, and some States.

Industry as a whole does not make a major investment in basic research because the results of such research cannot generally be patented and there industry cannot readily "profit" from basic research.

Program operations

All NSF research projects are carried out through grants and contracts. The large majority of the awards are made with scientists and engineers at colleges and universities. Over $500 million in the 1977 NSF budget will be awarded to private institutions. Grants and contracts are also awarded to private firms, non-profit organizations and State and local governments.

Awards are made on the basis of merit, through competition, and after intensive review of the research topic, project design and potential usefulness of the research results.

"National Priorities: Round One to Basic Science"

By William D. Carey

[From *Science Magazine,* Feb. 6, 1976, volume 191, p. 8]

In President Ford's new budget for fiscal year 1977, basic science has fared very well. An increase of 11 percent in federal support is provided, along with a 9 percent rise in R & D funds going to colleges and universities. Reversing the early tilt toward heavy reductions in R & D support, the President and his budget advisers have managed to provide an $800 million increase in civilian R & D in a total government budget that is notable for its austerity.

Several things can be said about the reasons for this unexpected outcome. The negotiations over the past year between the Congress and the White House, aimed toward restoring science policy machinery in the President's office, have helped to create a constructive policy dialogue on the role of government. The appointment of two distinguished White House advisory committees to brainstorm a science and technology policy agenda likewise has helped to broaden

the thinking of the President's staff. Reactions from the scientific community to the Administration's severe initial cutbacks in research support induced second thoughts in high places. The Office of Management and Budget itself came to believe that basic research support had been allowed to slip below the level of sufficiency and, encouraged by the Vice President, became a potent champion of a substantial budget increase. This convergence of concerned parties, coming at the issues from varying backgrounds, has made the difference in the outcomes. If this fragile policy system can be held in place for a time, we may be able to get on with building a workable public policy future for science and technology.

Still, the R & D budget for 1977 is a long way from being settled. A President proposes, but the Congress disposes. The Executive Budget expresses the President's intentions and preferences, but it does not bind the Congress. In an election year the budget is a national battleground, especially when the President and the legislative majority are on opposite political sides. It is also well to recall that Mr. Ford went to Congress last year with an increased National Science Foundation budget for basic research and failed to get it through. If this happens again with the 1977 budget, it is doubtful that the budgeteers will have much zest to try a third time.

The budget for R & D is at risk for other reasons, this year. The President's overall budget, of which R & D funding is only a small part, is very tight. It puts a moratorium on new policy initatives, and it cuts the "normal" 10 percent growth trend in half. It will dissatisfy those who prefer an expenditure policy which stimulates a slack national economy. It may not go down well to vote increases for basic science at the apparent expenses of more politically beneficial programs which have been held level or reduced. In short, the R & D budget is excellent on paper but is very vulnerable indeed. When the strife starts on Capitol Hill, an extraordinary effort will be necessary if the science budget is not to be turned into a shambles.

These are the realities. Science and technology are harnessed to an unstable and fractious public policy system whose behavior is erratic. Because the budget process is not well understood, yet plays a vital role in the progress of science and technology, the AAAS is initiating a special *Report on the Federal R & D Budget for 1977*, which will be issued late this spring. It will be a first stage assessment, attempting to illuminate the decision-making process and identify some of the critical issues in the 1977 budget. With guidance from the Committee on Science and Public Policy, the report will aim to isolate and describe the policy assumptions underlying major funding choices, tentatively explore the future implications of current decisions on research and development, and put down a foundation for what may become an annual AAAS White Paper on the Federal Budget for R & D. This is a large order, perhaps too large, but we will take it as far as we can. The hope is that this effort, along with others, will help to edge policy-makers, scientists, and engineers closer to a time when longrange policy strategies for R & D can make the Winter Olympics of budgeting less hazardous.

"The Ford Budget: New Signals for Science"

By William D. Carey

[From *Science Magazine*, Feb. 28, 1976, volume 187]

One embattled federal budget director used to say that budgets achieve only the uniform distribution of dissatisfaction. The 1976 budget, the first by President Ford, is in part the customary recital of griefs and apprehensions. But it also invites some small celebrations. Not the least of these is the substantial buildup in funds for research and development.

The new spending authority for federal R & D is scheduled to cross the $20 billion annual barrier in fiscal year 1976 and reach the level of $22.6 billion. Actual money outlays within fiscal 1976 will be somewhat less, but still a substantial $21.7 billion. The percentage rise from current levels is 15 percent in budget authority and 11 percent in outlays. True, the distribution pattern has a certain tiresome familiarity. Out of a $2.8 billion increase, $2.1 billion is for defense and space programs. About $0.7 billion represents the increase for civilian R & D, a good part of which is mortgaged to inflation.

Still, the trend is favorable to science and engineering. Rising costs are acknowledged. The needs for greater research effort in energy, food, and transportation have not been ignored. The National Science Foundation came through the grinder in generally good shape. An important beginning has been provided for research in climate dynamics, a sign of awareness of the food production implications of climate changes and their consequent impact on world economic and political stability.

There never was a perfect budget. This one is no exception. Its economic strategy appears too cautious for the kind of year we face, but it has the admirable merit of recognizing the future economic risks of overstimulation. Its defense posture starts us on a new military buildup while we are still warming to the idea of détente. In the area of civilian science, the budget increases are targeted to such particular problems as energy and do not benefit general-purpose science across the board. Health research funds will be tighter, and university-based research will find little to cheer about. In fact, the treatment of the education sector generally is uninspired and disheartening, with sharp cuts in support for library resources and educational development. Congress will no doubt have a second thought in some of these areas.

However, some optimistic signs can be read into this budget. Somebody must be listening to the science adviser; the NSF science advisory apparatus obviously helped the Office of Management and Budget shape the R&D portions of the new budget. Priority judgments are being made and reflected in the budget. The antiscience aura of the two previous administrations seems to have disappeared with the transfer of the White House reins. The Ford budget offers a long-delayed opportunity for government and science to begin working together toward a long-range public policy approach to science and technology.

As Vice President Rockefeller ponders changes in the White House staff system to more effectively utilize science and technology in Executive branch policy-making, we hope he will think in these long-range terms. Science and engineering have a responsible and effective role to play at the presidential level in coping with the problems of choice on which the future hangs. These include the transnational questions of the uses of the sea, the environment, and the resources of the planet, together with the uses of science and technology in creating alternative social and economic structures which can help reduce dissatisfactions leading to conflict. All of them mingle science and technology with public policy.

We think we see fresh signals for science in the 1976 budget, but the real test is more than a quantitative growth in R & D dollars. The corner will be turned when the budget for science and technology is thought of and expressed less as a set of annual expenditure decisions and more as an investment strategy which matches the scale and intensity of the nation's agenda.

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Section III—Effect of R.&D. in Private Industry

PART A—INDUSTRY INNOVATION, PRODUCTIVITY AND TECHNOLOGY

"Business and Technology: The Beautiful Bride or Wicked Stepmother

By Peter F. Drucker

From *Vital Speeches*, May 15, 1974, volume 40, p. 473.

Technology has been front page news for well over a century—and never more so than today. But for all the talk about technology, not much effort has been made to understand it or to study it, let alone to manage it. Economists, historians and sociologists all stress the importance of technology—but then they tend to treat it with "benign neglect," if not with outright contempt. (On this, see the "note" at the end of this paper.)

More surprisingly, business and businessmen have done amazingly little to understand technology and even less to manage it. Modern business is, to a very considerable extent, the creature of technology. Certainly the large business organization is primarily the business response to technological development. Modern industry was borne when the new technology of power generation—primarily water power at first—forced manufacturing activities out of home and workshop and under the one roof of the modern "factory," beginning with the textile industry in eighteenth century Britain. And the large business enterprise of today has its roots in the first "big business," the large railroad of the mid-nineteenth century, that is in technological innovation. Since then, the "growth industries," down to computer and pharmaceutical companies of today, have largely been the outgrowth of new technology.

At the same time, business has increasingly become the creator of technology. Increasingly, technological innovation comes out of the industrial laboratory and is being made effective through and in business enterprise. Increasingly, technology depends on business enterprise to become "innovation"—that is effective action in economy and society.

Yet business managers, or at least a very sizeable majority of them, still look upon technology as something inherently "unpredictable." Organizationally and managerially, technological activity still tends to be separated from the main work of the business and organized as a discrete and quite different "R and D" activity which, while in the business, is not really of the business. And until recently business managers, as a rule, did not see themselves as the guardians of technology and as concerned at all with its impact and consequence.[1]

[1] The one exception to this has been Japan all along, that is since the Meiji Restoration of 1867. From the early days, the Japanese saw technology clearly as part of economy and society and as something that deserved and required careful planning and deliberate management.

That that is no longer adequate should be clear to every business manager. It is indeed the thesis of this paper that business managers have to learn that technology is managerial opportunity and managerial responsibility. This means specifically:

(1) Technology is no more mysterious or "unpredictable" than developments in economy or society. It is capable of rational anticipation and demands rational anticipation. Business managers have to understand the dynamics of technology. At the very least, they have to understand where technological change is likely to have major economic impact and how to convert technological change into economic results.

(2) Technology is not separate from the business and cannot be managed as such if it is to be managed at all. Whatever role "R and D" departments or research laboratories play, the entire business has to be organized as an Innovative Organization and has to be capable of technological (but also of social and economic) innovation and change. This requires major changes in structure, in policies, and in attitude.

(3) The business manager needs to be concerned as much with the impacts and consequences of technology on individual, society and economy as with any other impacts and consequences of his actions. This is not talking "social responsibility"—that is responsibility for what goes on in society (e.g. minority problems). This is responsibility for impact of one's own actions. And one is always responsible for one's impact.

These last ten years there has been a widely reported "disenchantment with technology." It is by no means the first one in recent history (indeed similar "disenchantments" have occurred regularly every fifty years or so since the mid-eighteenth century). What is certain, however, is that technology will be more important in the last third of this century and will, in addition, change more than in the decades just past. Such great needs as the energy crisis, the environmental crisis, and the problems of modern urban society make this absolutely certain. Indeed one can anticipate, with high probability, that the next twenty-five years will see as much, and as rapid, technological change as in the "heroic age" of invention, the sixty years between the mid-nineteenth century and the outbreak of World War I. In that period, which began in 1856 with Perkins' discovery of aniline dyes and Siemens' design for the first workable dynamo, and which ended in 1911 with the invention of the vacuum tube and with it of modern electronics, today's "modern"—and even tomorrow's "post-modern" worlds were borne. In this "heroic age" a new major invention appeared on the average every fifteen to eighteen months, to be followed almost immediately by the emergence of a new industry based on it. The next twenty-five or thirty years, in all likelihood, will far more resemble this late nineteenth century period than the fifty years since the end of World War I which, technologically speaking, were years of refinement and modification rather than of invention. To the business and the businessmen who persist in the traditional attitude toward technology, the attitude which sees in it something "mysterious," something "outside," and something for which other people are responsible, technology will therefore be a deadly threat. But to busi-

ness and businessmen who accept that technology is *their* tool, but also their responsibility, technology will be a major opportunity.

ANTICIPATING AND PLANNING TECHNOLOGY

The "unpredictability" of technology is an old slogan. Indeed it underlies to considerable extent, the widespread "fear of technology." But it is not even true that *invention* is incapable of being anticipated and planned. Indeed, what made the "great inventors" of the nineteenth century—Edison, Siemens, or the Wright brothers—"great" was precisely that they knew how to anticipate technology, to define what was needed and would be likely to have real impact, and to plan technological activity for the specific breakthrough that would have the greatest technological impact—and, as a result, the greatest economic impact.

It is even more true in respect to "innovation" that we can anticipate and plan; indeed in respect to "innovation," we have to anticipate and plan to have any effect. And it is, of course, with "innovation" rather than with "invention" that the businessman is concerned. Innovation is not a technical, but a social and economic, term. It is a change in the wealth producing capacity of resources through new ways of doing things. It is not identical with "invention," although it will often follow from it. It is the impact on economic capacity, the capacity to produce and to utilize resources, with which "innovation" is concerned. And this is the area in which business is engaged.

It should be said that technology is no more "predictable" than anything else. In fact, predictions of technology are, at best useless and likely to be totally misleading. Jules Verne, the French science fiction writer of a hundred years ago, is remembered today because his predictions have turned out to be amazingly prophetic. What is forgotten is that Jules Verne was only one of several hundred science fiction writers of the late nineteenth century—which indeed was far more the age of science fiction writing than even the present decade. And the other 299 science fiction writers of the time, whose popularity often rivalled and sometimes exceeded that of Jules Verne, were all completely wrong. More important, however, no one could have done anything at the time with Jules Verne's predictions. For most of them, the scientific foundation needed to create the predicted technology did not exist at the time and were not coming into being for many years ahead.

For the businessman—but also for the economists or politicians—what matters is not "prediction," but the capacity to act. And this cannot be based on "prediction."

But technology can be anticipated. It is not too difficult—though not easy—to analyze existing businesses, existing industries, existing economies and markets to find where a change in technology is needed and is likely to prove economically effective. It is somewhat less easy, though still well within human limitations, to think through the areas in which there exists high potential for new and effective technology.[2]

[2] On this, see my book, *"Managing for Results"* (Harper & Row, 1964) especially chapters 10 and 11.

We can say flatly first that wherever an industry enjoys high and rising demand, without being able to show corresponding profitability, there is need for major technological change and opportunity for it. Such an industry can be assumed, almost axiomatically, to have inadequate, uneconomic, or plain inappropriate, technology. Examples of such industries would be the steel industry in the developed countries since World War II or the paper industry. These are industries in which fairly minor changes in process, that is fairly minor changes in technology, can be expected to produce major changes in the economics of the industry. Therefore, these are the industries which can become "technology prone." The process either is economically deficient or it is technically deficient—and sometimes both.

We can similarly find "vulnerabilities" and "restraints" which provide opportunities for new technology in the economies of a business and in market and market structure. The questions: What are the demands of customer and market which the present technology and the present business and the present technology do not adequately satisfy?" and: "What are the unsatisfied demands of customer and market?" that is the basic questions underlying market planning, are also the basic questions to define what technologies are needed, appropriate, and likely to produce economic results with minimum effort.

A particularly fruitful way to identify areas in which technological innovations might be both accessible and highly productive is to ask: "What are we afraid of in this business and in this industry? What are the things which all assert 'can never happen', but which we nonetheless know perfectly well might happen and could then threaten us? Where, in other words, do we ourselves know at the bottom of our hearts that our products, our technology, our whole approach to the satisfaction we provide to market and customer, is not truly appropriate and no longer completely serves its function?" The typical response of a business to these questions is to deny that they have validity. It is the responsibility of the manager who wants to manage technology for the benefit of his business and of his society to overcome this almost reflexive response and to force himself and his business to take these questions seriously. What is needed is not always new technology. It might equally be a shift to new markets or to new distributive channels. But unless the question is asked technological opportunities will be missed, will indeed be misconceived as "threats."

These approaches, of which only the barest sketch can be given in this paper, apply just as well to needs of the society as to needs of the market. It is, after all, the function of the businessman to convert need, whether of individual consumer or of the community, into opportunities for business. It is for the identification and satisfaction of that need that business and businessmen get paid. Today's major problems, whether of the city of the environment, or energy, are similar opportunities for new technology and for converting existing technology into effective economic action. At the same time, businessmen in managing technology also have to start out from the needs of their own business for new products, new processes, new services, to replace what is rapidly becoming old and obsolescent, that is to replace today. To identify technological needs and technological opportunities one

also starts out, therefore, with the assumption that whatever their business is doing today is likely to be obsolete fairly soon.[3]

This approach assumes a limited and fairly short life for whatever present products, present processes, and present technologies are being applied. It then establishes a "gap," that is the sales volume which products and processes not yet in existence will have to fill in two, five or ten years. It thus identifies the scope and extent of technological effort needed. But it also establishes what kind of effort is needed. For it determines why present products and processes are likely to become obsolescent, and it establishes the specification for their replacement.

Finally, to be able to anticipate technology, to identify what is needed and what is possible, and above all what is likely to be productive technology, the business manager needs to understand the dynamics of technology. It is simply not true that technology is "mysterious." It follows fairly regular and fairly predictable trends. It is not, as is often said, "science." It is not even the "application of science." But it does begin with new knowledge which is then, in a fairly well understood process, converted into effective—that is economically productive—application.

THE PACE OF TECHNOLOGY

It is often asserted today that technology is moving at a lightning pace, as compared with earlier times. There is no evidence for this assertion. It is equally asserted that new knowledge is being converted much faster into new technology than at any earlier time. This is demonstrably untrue. In fact, there is a good deal of evidence that it takes longer today to convert new knowledge, and especially new scientific knowledge, than it did in the nineteenth century—if not, indeed, in the eighteenth and earlier centuries. There is a lead time, and it is fairly long.

It took some twenty odd years from Siemens' design of the first effective dynamo to Edison's development of the electric light bulb, which first made possible an electrical industry. It has taken at least as long, in fact it has taken longer, from the design of the first working computer in the early forties to the establishment of truly producing computers—let alone to the development of the "software" without which a computer is (as was the early electric company) a "cost center" rather than a producer of wealth and economic assets. And there are countless similar examples. The lead time for the conversion of new knowledge into effective technology varies greatly between industries. It is perhaps shortest in the pharmaceutical industry. But even there, it is closer to ten years than to ten months. And, in any one industry, the lead time seems to be fairly constant.

What has shortened is the time between the introduction of new technology into the market and its general acceptance. There is less time to establish a pioneering position, let alone a leadership position. But even there, the "lead time" has not shortened as dramatically as most people, including most businessmen, assume. For both the electric light bulb and the telephone, that is for the 1880's, the lead time be-

[3] The best discussion of this approach, the planning approach from the needs of the business, is to be found in the essay by Michael J. Kami, *"Business Planning as Business Opportunity in Preparing Tomorrow's Business Leaders Today,"* edited by Peter F. Drucker (Prentice Hall, Englewood Cliffs, N.J., 1969). This essay, written for the symposium that celebrated the fiftieth anniversary of the Graduate School of Business Administration of New York University, sums up the experience of the two American companies which have most successfully innovated in technology in the period since World War II. For Mr. Kami, in the fifties and sixties, served first as Head of Long-Range Planning for IBM and then as Head of Long-Range Planning for Xerox.

tween a successful technological invention and widespread—indeed worldwide—acceptance was a few months. Within five years after Edison had shown his light bulb to the invited journalists, every one of the major electrical manufacturing companies in existence today in the Western world (excepting only Phillips in Holland) were established, in business, and leaders in their respective market. And the same held true for the telephone and for telephone equipment.

In other words, it is the job of the businessman to understand what new knowledge is becoming acceptable and available, to assess its possible technological impact, and to go to work on converting it into technology—that is into processes and products. He has to know, for this, not only the science and technology of his own field. Above all, he has to know that major technological "breakthroughs" very often, if not usually, originate in a field of science or knowledge that is different from that in which the old technology had its knowledge foundations. In this sense, the typical approach to "research," that is the approach for developing specialized expertise in the field in which one already is active, is likely to be a bar to technological leadership rather than its main pillar, as is commonly believed. What is needed, at least as a complement, is the ability to scan the horizon of knowledge, to be alert to new insights and new awareness, and to be able to see their potential application to one's own field. What is needed, in other words, is a "technologist," rather than a "scientist." And often a layman, with good "feel" for science and technology, and with genuine intellectual interest, does this much better than the highly trained specialist in a technical or a scientific field—who is likely to become the prisoner of his own advanced knowledge.

It is not necessary, it is indeed not even desirable, for the businessman to become a "scientist" or even a "technologist." His role is to be the manager of technology. This requires an understanding of the process of technology and of its dynamics. It requiries willingness to anticipate tomorrow's technology and, above all, willingness to accept that today's technology with its processes and products is becoming obsolete rapidly. It requires identifying the needs for new technology and the opportunities for it, in the vulnerabilities and restraints of the business, in the needs of the market and in the needs of the society. Above all, it requires acceptance of the fact that technology had to be considered a major business opportunity which, to identify and to exploit, is what the businessman is paid for.

The next quarter century, as has already been said, is likely to require innovation and technological change as great as any we have ever witnessed. Most of this, however, in sharp contrast to the nineteenth century, will have to be done in and by established organizations, and especially in and by established businesses.

It is not true, as is often said, that "big business monopolizes innovation." On the contrary, the last twenty-five years have been preeminently years in which small businesses and often new and totally unknown businesses produced a very large share of the most effective innovations. Xerox was nothing but a small paper merchant as late as 1950. Even IBM was still a small company and a mere pygmy, even in its own office equipment industry, as late as World War II. Most

of today's pharmaceutical giants were either small companies at the end of World War II or barely in existence, and so on.

But still, increasingly, the major effort in technological change is development and the effort of market introduction. These do not require "genius." They require highly educated people in a massive cooperative effort. And they require very large sums of capital. And these are indeed found in established institutions, whether business or government.

Altogether the existing businesses will have to become innovative organizations. For the last fifty to seventy-five years our emphasis has, properly, been on managing what we already know and understand. For the pace of technological innovation—and even the pace of economic change—in these last seventy-five years was, contrary to popular belief, singularly slow.[4]

Now business will again have to become entrepreneurial. And the entrepreneurial function, as the greatest of Continental European economists, J. B. Say (1767–1832) saw clearly almost two centuries ago, is to move existing resources from areas of lesser productivity to areas of greater productivity. It is to create wealth not by discovering new continents, but by discovering new and better uses for the existing resources and for the known and already exploited economic potentials. And technology, while not the only tool for this purpose, is an important one and may well be the most important one.

The great task of business can be defined as counteracting the specific "law of entropy" of any economic system: the law of the diminishing productivity of capital. It was on this "law" that Karl Marx based his prediction of the imminent demise of the "capitalist system." Yet capital has not only not become less productive, it has steadily increased its productivity in the developed countries—contrary to the assumed "law." But Karl Marx was right in his premise. Left to its own devices, any economy will indeed move toward steadily diminishing productivity of capital. The only way to prevent it from becoming entropic, the only way to prevent it from degenerating into sterile rigidity, is the constant renewal of the productivity of capital through entrepreneurship—that is through moving resources from less productive into more productive employment. This, therefore, makes technology the more important the more highly developed technologically a society and economy become.

In the next twenty-five years, when the world will have to grapple with a population problem, an energy problem, a resources problem, and a problem of the basic community, that is the city, this function is likely to become increasingly more critical—independent, by the way, of the political, social, or economic structure in a developed economy, that is independent of whether the "system" is "capitalist," socialist," "communist," or something else.

This will require businessmen to learn how to build and how to manage an innovative organization.[5] Normally, the innovative organization is being discussed in terms of "creativity" and of "attitude."

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[4] On this, see my book *"The Age of Discontinuity"* (Harper & Row, 1969), especially chapters 1 or 2.

[5] On this, see my book, *"Management: Tasks; Responsibilities; Practices"* (Harper & Row, 1974), especially Chapter 61.

What it requires, however, are policies, practices and structure. It requires, first, that management anticipate technological needs, identify them, plan for them, and work on satisfying them.

It requires, secondly, and perhaps most importantly, that management systematically abandon yesterday.

"Creativity" is largely an excuse for doing nothing. The problem in most organizations which are incapable of innovation and self-renewal is that they cannot slough off the old, the outworn, the no longer productive. On the contrary, they tend to allocate to it their best resources, especially of good people. And anybody incapable of eliminating . . . waste products poisons itself eventually. What is needed to make an innovative organization is a systematic policy for abandoning the no longer truly productive, the no longer truly contributing. The innovative organization requires, above all, that every product, every process, every activity, be put on "trial for its life" periodically—maybe every two or three years. The question should be asked: "If we did not do this already, would we now—knowing what we now know—go into it?" And if the answer is, "No," then one does not ask: "Should we abandon it?" Then one asks: "How can we abandon it, and how fast?"

The organization, whether business, university, or government agency, which systematically sloughs off yesterday need not worry about "creativity." It will have such a healthy appetite for the new that the main task of management will be to select among the large number of good ideas for the new, the ones with the highest potential of contribution and the highest potential of successful completion.

But beyond this, the innovative organization needs specific policies. It needs measurement and information systems which are appropriate to the economic reality of innovation—and a regular, moderate and continuous "rate of return on investment" is the wrong measurement. Innovation, by definition, is only cost for many years before it produces a "profit." It is first an investment—and a return only much later. But that also means that the rate of return must be far larger than the highest "rate of return" for which managers plan in a managerial type of business. Precisely because the lead time is long and the failure rate high, a successful innovation in an innovative organization must aim at creating a new business with its potential for creating wealth, rather than a nice and pleasant addition to what we already have and what we already do.

Finally, we will have to realize that innovative work is not capable of being organized and done within managerial components, that is units concerned primarily with work on today and on tomorrow morning. It needs to be organized separately, with different structural principles and in different structural components. Above all, the demand on managerial self-discipline and on clarity of direction and objectives are much greater in innovative work and have to be extended to a much larger circle of people. And therefore, the innovative organization, while organically a part of the ongoing business needs to be structurally and managerially separate. Businessmen, to be able to build and lead innovative organizations will, therefore, have to be able to do both—manage what is already known and create the new and unknown. They will have to be able both to optimize the existing business and to maximize the potential business.

These, to most businessmen, are strange and indeed somewhat frightening ideas. But there are plenty of truly innovative businesses around—in practically every country—to show that the task can be done, and is indeed eminently do-able. In fact, what is needed primarily is recognition—lacking so far in most management thinking and in almost all management literature—that the innovative organization is a distinct and different organization, and is not only a slightly modified managerial organization.

RESPONSIBILITY FOR THE IMPACT OF TECHNOLOGY

Everybody is responsible for the impact of his actions. This is one of the oldest principles of the law. It applies to technology as well. There is a great deal of talk today about "social responsibility." But surely the first point is not responsibility for what society is doing, but responsibility for what one is doing oneself. And therefore, technology had to be considered under the aspect of the businessman's responsibility for the social impacts of his acts. In particular, there is the question of the "by-product impacts," that is the impacts which are not part of the specific function of a process or product but are, necessarily or not, occurring without intention, without adding to the intended or wanted contribution, and indeed as an additional cost—for every by-product which is not converted into a "saleable product" is, in effect, a waste and therefore a cost.

The topic of the responsibility of business for its social impact is a very big one.[6] And the impacts of technology, no matter how widely publicized today, are among the lesser impacts. But they can be substantial. Therefore the businessman has to think through what his responsibilities are and how he can discharge them.

TECHNOLOGY ASSESSMENT

There is, these days, great interests in "Technology Assessment," that is in anticipating impact and side effects of new technology *before* going ahead with it. The U.S. Congress has actually set up an "Office of Technology Assessment." This new agency is expected to be able to predict what *new technologies* are likely to become important, and what long-range effects they are likely to have. It is then expected to advise Government what new technologies to encourage and what new technologies to discourage, if not to forbid altogether.

This attempt can only end in fiasco. "Technology Assessment" of this kind is likely to lead to the encouragement of the wrong technologies and the discouragement of the technologies we need. For *future* impacts of *new* technology are almost always beyond anybody's imagination.

DDT is an example. It was synthesized during World War II to protect American soldiers against disease-carrying insects, especially in the tropics. Some of the scientists then envisaged the use of the new chemical to protect civilian populations as well. But not one of the many men who worked on DDT thought of applying the new pesticide to control insect pests infecting crops, forests or livestock. If DDT had

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[6] On this, see especially my book, *"Management: Tasks; Responsibilities; Practices"* (Harper & Row, New York, 1974), especially chapter 25.

been restricted to the use for which it was developed, that is to the protection of humans, it would never have become an environmental hazard; use for this purpose accounted for no more than 5 to 10 percent of the total at DDT's peak, in the mid-sixties. Farmers and foresters, without much help from the scientists, saw that what killed lice on men would also kill lice on plants, and made DDT into a massive assault on the environment.

Another example is the "population explosion" in the development countries. DDT and other pesticides were a factor in it. So were the new antibiotics. Yet the two were developed quite independently of each other; and no one "assessing" either technology could have foreseen their convergence—indeed no one did. But more important as causative factors in the sharp drop in infant mortality, which set off the "population explosion," were two very old "technologies" to which no one paid any attention. One was the elementary public health measure of keeping latrine and well apart—known to the Macedonians before Alexander the Great. The other one was the wire-mesh screen for doors and windows invented by an unknown American around 1860. Both were suddenly adopted even by backward tropical villages after World War II. Together they were probably the main "causes" of the "population explosion."

At the same time, the "technology impacts" which the "experts" predict almost never occur. One example is the "private flying boom." which the experts predicted during and shortly after World War II. The private planes, owners-piloted, would become as common, we were told, as the "Model T" automobile had become after World War I. Indeed, "experts" among city planners, engineers and architects advised New York City not to go ahead with the second tube of the Lincoln Tunnel, or with the second deck on the George Washington Bridge; and instead to build a number of small airports along the west bank of the Hudson River. It would have been fairly elementary mathematics to disprove this particular "technology assessment"—there just is not enough airspace for commuter traffic by air. But this did not occur to any of the "experts"; no one then realized how finite airspace is. At the same time, almost no "experts" foresaw the expansion of commercial air traffic and anticipated, at the time the jet plane was first developed so that it would lead to mass transportation by air, with as many people crossing the Atlantic in one jumbo jet twelve times a day as used to go once a week in a big passenger liner. To be sure, transatlantic travel was expected to grow fast—but of course it would go by ship. These were the years in which all the governments along the North Atlantic heavily subsidized the building of new super-luxury liners just when the passenger deserted the liner and switched to the new jet plane.

A few years later, we were told by everybody that "automation" would have tremendous economic and social impacts—it has had practically none. The computer offers an even odder story. In the late forties nobody predicted that the computer would be used by business and governments. While the computer was a "major scientific revolution," everybody "knew" that its main use would be in science and warfare. As a result, the most extensive market research study undertaken at that time reached the conclusion that the world computer market would, at most, be able to absorb 1,000 computers by the year 2000.

Now, only twenty-five years later, there are some 150,000 computers installed in the world, most of them doing the most mundane bookkeeping work.

Then a few years later, when it became apparent that business was buying computers for payroll or billing, the "experts" predicted that the computer would displace "middle management," so that there would be nobody left between the Chief Executive Officer and the Foreman. "Is middle management obsolete?" asked a widely quoted Harvard Business Review article in the early fifties; and it answered this rhetorical question with a resounding, "Yes." At exactly that moment, the tremendous expansion of middle management jobs began. In every developed country middle management jobs, in business as well as in government, have grown about three times as fast as total employment in the last twenty years; and their growth has been parallel to the growth of computer usage.

Anyone depending on "technology assessment" in the early fifties would have abolished the graduate business schools as likely to produce graduates who could not possibly find jobs. Fortunately, the young people did not listen and flocked in record numbers to the graduate business schools so as to get the good jobs which the computer helped to create.

But while no one foresaw the computer impact on middle management jobs, every "expert" predicted a tremendous computer impact on business strategy, business policy, planning, and top management—on none of which the computer has, however, had the slightest impact at all. At the same time, no one predicted the real "revolution" in business policy and strategy in business in the fifties and sixties, the merger wave and the "conglomerates."

DIFFICULTY OF PREDICTION

It is not only that Man no more has the gift of prophecy in respect to technology than in respect to anything else. The impacts of technology are actually more difficult to predict than most other developments. In the first place, as the example of the "population explosion" shows, social and economic impact are almost always the result of the convergence of a substantial number of factors, not all of them technological. And each of these factors has its own origin, its own development, its own dynamics, and its own experts. The "expert" in one field, e.g. the expert on epidemiology never thinks of plant pests. The expert on antibiotics is concerned with the treatment of diseases—whereas the actual explosion of the birthrate largely resulted from elementary and long-known public health measures.

But equally important, what technology is likely to become important and have an impact, and what technology either will fizzle out—like the "flying Model T"—or will have minimal social or economic impacts—like automation—is impossible to predict. And which technology will have social impacts and which will remain just technology, is even harder to predict. The most successful prophet of technology, Jules Verne, predicted a great deal of twentieth century technology a hundred years ago (though few scientists or technologists of that time took him seriously). But he anticipated absolutely no social or economic impacts, but an unchanged mid-Victorian

society and economy. Economic and social prophets, in turn, have the most dismal record as predictors of technology.

The one and only effect an "Office of Technology Assessment" is therefore likely to have would be to guarantee full employment to a lot of fifth-rate science fiction writers.

THE NEED FOR TECHNICAL MONITORING

However, the major danger is that the delusion that we can foresee the "impacts" of new technology will lead us to slight the really important task. For technology does have impacts and serious ones, beneficial as well as detrimental ones. These do not require prophecy. They require careful monitoring of the actual impact of a technology once it has become effective. In 1948 practically no one correctly saw the impacts of the computer. Five and six years later, one could and did know. Then one could say: "Whatever the technological impact, *socially* and *economically* this is not a major threat." In 1943 no one could predict the impact of DDT. Ten years later, DDT had become a world-wide tool of farmer, forester and livestock breeder, and as such, a major ecological factor. Then, thinking as to what action to take should have begun, work should have been started on the development of pesticides without the major environmental impact of DDT, and the difficult "trade-offs" should have been faced between food production and environmental damage—which neither the unlimited use nor the present complete ban on DDT sufficiently considers.

"Technology monitoring" is a serious, an important, indeed a vital task. But it is not "prophecy." The only thing possible, in respect to *new* technology, is *speculation* with about one chance in a hundred of being right—and a much better chance of doing harm by encouraging the wrong, or discouraging the most beneficial new technology. What needs to be watched is "developing" technology, that is technology which has already had substantial impacts, enough to be judged, to be measured, to be evaluated.

And "monitoring" a "developing" technology for its social impacts is, above all, a managerial responsibility.

But what should be done, once such an impact has been identified? Ideally, it should be eliminated? Ideally the fewer impacts, the fewer "costs" are being incurred, whether actual business costs, externalities, or social costs. Ideally, therefore, businesses start out with the commitment to convert the elimination of such an impact into "business opportunity."

And where this can be done, the problem disappears, or rather it becomes a profitable business and the kind of contribution for which business and businessmen are properly being paid. But where this is not possible, business should have learned, as a result of the last twenty years, that it is the task of business to think through what kind of regulation is appropriate. Sooner or later, the impact becomes unbearable. It does no good to be told by one's public relations people that the "public" does not worry about the impact that it would, in fact, react negatively toward any attempt to come to grips with it. Sooner or later, there is then a "scandal." The business which has not worked on anticipating the problem and of finding the right solution, that is the right regulation, will then find itself both stigmatized and penalized—and properly so.

This is not the popular thing to say. The popular thing is to assert that the problems are obvious. They are not. In fact, anyone who would have asked for regulation to cut down on air pollution from electric powerplants twenty or even ten years ago, would have been attacked as an "enemy of the consumer" and as someone who, "in the name of profit," wanted to make electricity more expensive. (Indeed this was the attitude of regulatory commissions when the problem was brought to their attention by quite a few power companies.) When the Ford Company in the early fifties introduced seat belts, it almost lost the market. And the pharmaceutical companies were soundly trounced by the medical profession every time they timidly pointed out that the new high-potency drugs required somewhat more knowledge of pharmacology, biology and biochemistry than most practicing physicians could be expected to have at their disposal.

But these examples also, I think, bring out that the "public relations" attitude is totally inappropriate and, in fact, self defeating. They bring out that neglect of the impact and willingness to accept that "nobody is worried about it," in the not-so-very-long-run penalizes business far more seriously than willingness to be unpopular could possibly have done.

Therefore, in technology-monitoring the businessman not only has to organize an "early warning" system to identify impacts, and especially unintended and unforeseen impacts. He then has to go to work to eliminate such impacts. The best way, to repeat, is to make the elimination of these impacts into an opportunity for profitable business. But if this cannot be done, then it is the better part of wisdom to think through the necessary public regulation and to start early the education of public, government, and also of one's own competitors and colleagues in the business community. Otherwise the penalty will be very high—and the technology we need to tackle the central problems of "post-industrial" society will meet with growing resistance.

CONCLUSION

Technology is certainly no longer the "Cinderella" of management which it has been for so long. But it is still to be decided whether it will become the beautiful and beloved bride of the "prince," or instead turn into the fairytale's "wicked stepmother." Which way it will go, will depend very largely on the business executive and his ability and willingness to manage technology. But which way it will go will also very largely determine which way business will go. For we need new technology, both major "breakthroughs" and the technologically minor but economically important and productive changes to which the headlines rarely pay attention. If business cannot provide them, business will be replaced as a central institution—and will deserve to be replaced. Managing technology is no longer a separate and subsidiary activity that can be left to the "longhairs" in "R and D." It is a central management task.

A HISTORICAL NOTE

The absence of any serious concern and study of technology among the major academic disciplines is indeed puzzling. In fact, it is so puzzling as to deserve some documentation.

The nineteenth century economist usually stressed the central importance of technology. But he did not go beyond paying his elaborate respects to technology. In his system he relegated technology to the shadowy limbo of "external influences," somewhat like earthquakes, locusts or wind and weather, and as such incomprehensible, unpredictable, and somehow not quite respectable. Technology could be used to explain away phenomena which did not fit the economist's theoretical model. But it could not be used as part of the model. The twentieth century Keynesian economist does not even make the formal bow to technology which his nineteenth century predecessor regarded as appropriate. He simply disregards it. There are, of course, exceptions. Joseph Schumpeter, the great Austro-American economist, in his first and best known work on the dynamics of economic development put the "innovator" into the center of his economic system. And the innovator in large part was a technological innovator. But Schumpeter found few successors. Among living economists only Kenneth Boulding at the University of Colorado seems to pay any attention to technology. The ruling schools, whether Keynesian, neo-Keynesian, or Friedmanite, pay as little attention to technology as the preindustrial schools of economists, such as the Mercantilists before Adam Smith. But they have far less excuse for this neglect of technology.

Historians, by and large, have paid even less attention to technology than economists. Technology was more or less considered as not worthy the attention of a "humanist." Even economic historians have given very little attention to technology until fairly recently. Interest in technology as a subject of study for the historian did not begin until Lewis Mumford's book, "*Technics and Civilizations*" (in 1934). It was not until twenty-five years later that systematic work on the study of the history of technology began, with publication in England in 1957/58 of "*A History of Technology*," edited by Charles Singer (five volumes, Oxford, 1957/58) : and simultaneously in the United States with the founding of the Society for the History of Technology in 1958 and of its journal. Technology and Culture. The relationship between technology and history has further been discussed in the first American text book: "*Technology in Western Civilization*," edited by Melvin Kranzberg and Carroll W. Pursell, Jr. (2 volumes, Oxford, 1967), and in my essay volume: "*Technology, Management and Society*" (Haper & Row, New York, 1970), (especially in the essays, "*Work and Tools*" (written in 1959) : "*The Technological Revolution*": "*Notes on the Relationship of Technology, Science and Culture*" (1961) ; and "*The First Technological Revolution and Its Lessons*," first delivered as a Presidential Address to the Society for the History of Technology in December, 1965.) The California Medievalist, Lynn White, Jr., has done pioneering work on the impact of technological changes on society and economy, especially in his book," *Medieval Technology and Social Change*" (Oxford, 1962). But the only work that tries successfully to integrate technology into history, especially economic history, is the recent book by the Harvard economic historian, David S. Landes, "*The Unbound Prometheus; Technological Change and Industrial Development in Western Europe; 1750 to the Present*," (1969). Outside of the English-speaking countries only one historian of rank has given any attention to technology, the German

Franz Schnabel in his "*Deutsche Geschichte in 19 Jahrhundert*" (Friedurg, 1929–1937).

Perhaps even more perplexing is the attitude of the sociologist. While the word "technology" goes back to the seventeenth century, it first became a widely used term as slogan if not as manifesto of the early sociologists in the late eighteenth century. To call the first technical university in 1794 "*Ecole Polytechnique*" was for instance a clear declaration of basic principles and, above all, a declaration of the central importance of technology to society and social structure. And the early fathers of sociology, especially the great French sociologists, Saint-Simon and Auguste Comte, did indeed see technology as the great liberating force in society. Marx still echoes some of this—but then relegates technology to the realm of secondary phenomena. Sociologists since then have tended to follow Marx and to put the emphasis on property relationships, kinship relationships and on everything else, but not on technology. There are plenty of slogans such as that of "alienation." But there has been practically no work done. And technology is barely mentioned in the major sociological theories of the last, that is the post-Marx, century from Max Weber to Marcuse and Levy-Bruhl to Levy-Strauss and Talcott Parsons. Technology either does not exist at all for the sociologist, or it is an unspecified "villain."

In other words, the scholars have yet to start work on technology, as the way Man works; as the extension of the limited physical equipment of the biological creature that is Man; as a part—a major part—of Man's intellectual history and intellectual achievement; and as a human achievement which, in turn, influences the human condition profoundly. However, the businessman cannot wait for the scholars. He has to manage technology now.[7]

"The Silent Crisis in R&D"

From *Business Week*, Mar. 8, 1976, p. 90.

Even if the apparent economic recovery takes a firm hold, some concerned economists are warning that the U.S. may face a serious slowdown in its long-term growth rate because of the protracted slump in spending for research and development.

Merton Peck of Yale calls it "the silent crisis"—not obvious like other national crises but nonetheless real. Harvard economist Zvi Griliches, one not given to hyperbole, says: "The slack growth of the past seven years in research and development spending will come home to roost."

Economists in this group maintain that the slowdown in long-term growth has already been set in motion by the slump in R&D spending since the late 1960s (chart). They have no pat solutions to the problem, and their new findings raise almost as many questions as they answer. But their message is clear: unless the nation steps up its

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[7] For more examples of the complexity of the impact problem, see the section, "Three Cautionary Tales" in Chapter 24 of my book, "*Management: Tasks; Responsibilities; Practices*" (Harper & Row, New York, 1974).

investment in developing and marketing improved processes and products, the U.S. economy is doomed to grow more slowly in the 1970s and 1980s than it did in the 1960s. The potential loss in gross national product in the next 10 years alone could reach $100 billion, not counting the loss of improvement in the quality of life that cannot be measured in GNP dollars.

Moreover, productivity gains, which reduce unit costs, are a prime factor in combating long-term inflation. Thus, by scrimping on R&D, the nation is fighting the inflation battle with its right arm in a sling.

Losing ground fast. The slowdown in research spending has been dramatic. From 1953 to 1961, R&D expenditures, adjusted for inflation, increased at an average rate of 13.9% a year for government and 7.7% for nongovernment, according to the National Science Foundation. From 1961 to 1967, government-funded R&D increased 5.6% a year and private R&D 7.4%. But from 1967 to 1975, government R&D shrank 3% a year, and nongovernment spending rose a mere 1.8% a year.

Recent studies confirm not only that R&D is a significant factor in the nation's growth but also that companies themselves are missing a good bet by not putting more of their dollars into R&D. The major findings:

Organized R&D projects of the kind that are covered in company budgets account for about 40% of the total increase in U.S. productivity. This means that a dollar spent on R&D has a far greater impact on economic growth than a dollar invested in physical capital.

Industry earns an average 30% rate of return per year on its R&D spending—about twice the return that companies get from their capital investments.

On the average, the largest companies do not spend proportionately more or less than smaller companies, and their rate of return per dollar of R&D expenditure is also similar. Therefore, tinkering with the market structure of U.S. industry to spur R&D spending offers no hope of a payoff.

Three sources feed the stream of national economic growth: capital investments, increases in both the quantity and quality of the labor force, and improvements in technology. Although economists have long labored over the perplexities in measuring the contribution of each of these three factors, they now agree that technological gains have been as important to the economic growth of the U.S. as increases in capital and labor. Fully one-third of the measured growth in GNP has come from technological progress, they say.

The payoff. Technological progress stems from a variety of sources, including such informal ones as the amateur inventor puttering in his garage on weekends or the manager who gets an idea on how to organize the production line better. The major question is how much of this progress has resulted directly from organized R&D. Griliches' econometric findings put this contribution at 20% to 25%. And a recent study by Nestor E. Terleckyj of the National Planning Assn. conservatively estimates that in 1948–66 organized R&D was responsible for at least 33% of the improvement in technology. Considering that industry invests about 10 times as much in plant as in R&D, $1 of R&D has almost four times the impact on growth that $1 invested in plant and equipment has.

Spending on R&D has not only paid off in a big way for the country as a whole but has also produced high rates of return for the industries that do the spending. A paper by Griliches shows that in 1957–65 R&D produced an average annual 27% rate of return, based on the depreciated life of the expenditure. This, he says, is about twice the return on physical capital investments by the 883 companies in his sample.

Using a different set of data, Terleckyj finds a direct productivity return of about 30% a year on R&D in manufacturing industries in 1948–66. Moreover, he finds, some industries that do little of their own R&D but buy from R&D-intensive industries achieved productivity gains of 80% per year. For example, airlines benefit from the R&D done by the airframe industry.

The findings of Griliches and Terleckyj apply only till the mid-1960s, the latest years for which detailed data are available. But a study by Edwin Mansfield of the Wharton School seems to indicate that high rates of return persisted into the 1970s. Mansfield used a sample of 17 run-of-the-mill innovations, ranging from a metal process introduced in the late 1950s to a door control mechanism marketed in the early 1970s. He shows an average annual rate of return of 25% to the innovating company, after adjustment for unsuccessful R&D efforts, and a return of well over 50% to other users as a whole.

Why the slump? Given the high rates of return on R&D, economists are hard-pressed to explain why industry spending has lost its historically robust growth. Industry R&D amounted to 1.5% of total sales in 1957; peaked at 2.3% in 1969, and has now fallen below 2%.

Some businessmen contend that increased government regulation of industry has a crimp in private R&D. But Terleckyj argues that it should only open vistas for new and better products and would not have an overall negative impact. Some critics suggest that the oligopolistic market structure of U.S. industry is the cause. But economists who have analyzed the relationship between company size and technological progress deny this claim.

There are two conflicting views of the impact of big companies on innovation. One theory, first formulated by the late Joseph Schumpeter and eloquently elaborated upon by John Kenneth Galbraith, holds that R&D is carried out much more productively by very large companies. To foster innovation, this theory would call for relaxation of the antitrust laws. In total contrast, some economists and most consumer advocates maintain that large companies stifle innovation, so antitrust laws should be enforced more vigorously.

The overwhelming evidence is that neither theory fits the facts. "For practically all industries," Wharton's Mansfield says, "the data indicate that very big firms do not carry out more innovation relative to their size than do smaller firms." Instead, economists find a threshold effect. Explains Mansfield: "In order to do much research and development, a firm must be of a certain minimum size—which, of course, will vary by industry. Beyond that, innovation seems to be proportional to size. He does note exceptions, though. For example, Du Pont spends more on R&D, relative to its size, than smaller chemical companies do.

The very largest companies may get slightly less productivity out of an R&D dollar than somewhat smaller companies get, Mansfield

adds, but this disadvantage is offset by their ability to do a superior marketing job. He says: "Small and large firms should be looked at as complements to each other. You need the small firm's flexibility to move quickly, especially in the initial stages of innovation, and you need the very large firms with their huge capital resources, large numbers of researchers, and marketing knowhow."

How to cash in. "R&D isn't worth anything alone," Mansfield maintains. "It has got to be coupled with the market. The innovative firms are not necessarily the ones that produce the best technical output but the ones that know what is marketable."

Economists who agree cite as an extreme case the Soviet Union's problems in improving productivity through R&D. "They keep spending tons of money on R&D, but they don't get much out of it," says a government economist who specializes in Soviet affairs. "The Soviets are right at the frontiers of research in the lab. It's getting from the lab to metal-bending and into production that is the trick."

Charlotte Schroeder of the University of Virginia, an expert on the planned economies, notes that the Russians are turning out engineers and scientists by the drove. But because of the emphasis on today's production, managers have little incentive to shut down for retooling and, thus, are not receptive to new ideas. "The researchers and the production people are on different wavelengths," she says.

Resistance to innovation is not confined to planned economies, Mansfield finds. In a survey of the R&D executives of 20 major chemical, drug, and electronics companies in the U.S., he found an opinion that the success rate of innovation would have been boosted by 50% if the R&D results had been fully and properly exploited by their companies' manufacturing and marketing people. To confirm this evaluation, Mansfield put the same set of questions to non-R&D executives. Their estimate turned out to be even higher than that of the R&D managers.

Incentives. While economists agree that companies should invest more heavily in R&D and should improve their output from it, by no means all of them advocate that government should provide special tax incentives. "Since R&D really is investment, there's already a tax benefit because companies can write it all off," says William Fellner, of the American Enterprise Institute. And Yale's Merton Peck says: "I'm against government incentives. The best thing to get private R&D moving is get this economy off its bottom."

But Mansfield disagrees. "We need a general incentive," he suggests. "Let's explore tax incentives carefully." Edward F. Denison of the Brookings Institution, a pioneer in the study of economic growth, says: "I have always been against tax incentives for R&D. But after seeing Mansfield's work on the rate of return, I'm reconsidering my position."

Although Fellner is against tax incentives, he favors more federal funds for R&D, especially where the risks for private industry are enormous, as in energy development. In the 1977 budget, federal funds for R&D increase only 10% in inflation-bloated dollars, and Fellner notes that real R&D will rise less than real GNP. "I would not try to save on the R&D budget," he says.

Griliches believes that more federal dollars should be going into the universities, where he says the nation has a high-class scientific establishment that is "now underutilized and malnourished." The low rate

of utilization, he concedes, is partly the universities' own fault. "They expanded, thinking that government funds would be flowing forever," he says. Still, he says, university labs represent an underutilized resource that should be put to work.

Like Fellner, Mansfield argues for more federal R&D funds, but he also does not advocate opening all the money taps. "I'm not for throwing a wad of money into machine tool research, for instance," he says. "What we do need is a concerted research effort to determine just where government R&D funds should be going."

"Federal Support of Commercially Relevant R&D"

By Lewis M. Branscomb

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For observers of the science and technology policy scene, 1971 was an exciting year. For ten years an idea had been smoldering in Washington, an idea first launched during the Kennedy Administration by Herb Hollomon, Milton Harris, and Jerome Wiesner. Triumphant in World War II, the United States in the early 1960s found itself caught up in the missile, nuclear weapons, and space competitions with the Soviet Union; little attention was being given to the need to channel research and development resources to the civil economy. Hollomon took action on the basis of Harris's Presidential task force report and established a Civilian Industrial Technology Program in the Department of Commerce.

Like most of Hollomon's good ideas, this one was at least as much ahead of its time as some industry lobbyists and congressional committees were behind. You can read the obituary of the CIT Program in Dorothy Nelkin's little book *The Politics of Housing Innovation* [1]. But despite the controversy that surrounded the program, its content was very modest. A few million dollars were made available to engineering schools engaged in textile research to permit them to collaborate with the industry in the quest for basic technological improvements that would help the whole industry. The wraths with which some industry spokesmen derided this effort as an attempt to subvert free enterprise makes you wonder if they know what the federal government was doing for the civil aviation and nuclear power industries at the same time. Far from providing only a few academic, applied research projects on problems common to the industry, the federal government was sustaining the early-phase product-development cycle of a select group of manufacturers as a means for compensating the large risks involved.

It was clear that the federal government had no consistent policy in regard to the use of public funds to support the growth of the economy through assistance to industrial technology. The *de facto* policy seemed to be: The government would accept the primary responsibility for developing new industries if the entry cost was unacceptable to private capital *and* a plausible connection could be made between the strength of the industry and the national defense, thus justifying a federal role. For less capital-intensive industries the government would provide a modest but important level of central technical services through the

in-house work of the NBS and would provide a more extensive set of both research data and education programs in agriculture. But most of this work would be done in not-for-profit or government facilities to minimize any objection that a differential advantage was being given to one competitor among many.

With the passage of time, the close partnership between industrial trade associations and the NBS for the conduct of research of interest to all had begun to change. Leading firms had acquired enough research capability to prefer their exclusivity to joint efforts that were shared with others. Others were depending on the contracts available from the defense and atomic energy establishments, which led them down noncommercial paths. The remainder observed NBS backing away from traditional industrial research areas—glass, concrete, fibers, tires, lubricants, and paper—and concentrating on the refurbishing of basic scientific strengths that had been allowed to deteriorate during the war.

During the decade of the 1960s the aerospace industry had few causes for complaints about federal R&D policy. The traditional industries were more interested in less government regulation than in more government technical help. The academic community reinforced a national policy for the encouragement of science that made little effort even to address civil technology. What happened, then, to stimulate the 1971 policy debate in which Bill Magruder, Ed David, and George Schultz played leading roles, a debate which began in a Domestic Council task force in July 1971 and whose resolution took form in the State of the Union Budget and Economic messages of January 1972 and the historic Science and Technology message of March 1972?

Five circumstances built new interest around the industrial technology issue:

(1) The persistence of a deteriorating deficit in the balance of payments, despite monetary readjustments in Germany and Japan, was a clear symptom that the international competitive standing of the U.S. economy was under serious challenge abroad. Although several administration leaders, including Commerce Secretary Stans, sought to bring to the President's attention the importance of technology in economic competition, in the context of a worsening imbalance of payments, it was White House Special Assistant Peter Petersen whose report focused the issues. Those who were carried away promptly invented a "reverse technology gap."

(2) Heavy unemployment among engineers caused by cutbacks in federal funding of aerospace research and development projects brought new recruits to the "reverse technology gap" hypothesis and brought calls for reinvesting the manpower into civil programs. It was generally conceded that domestic priorities were almost nowhere mirrored in federal R&D priorities.

(3) But many policy leaders were already unimpressed by the value to the economy of commercial technologies that were alleged to "spin off" from aerospace engineering. The NASA Technology Utilization Program represented a major effort to foster such spin-offs, but the program's occasional successes served only to emphasize the overall futility of looking to space exploration as a means of stimulating commercial technologies. If federally funded R&D was to make a

major contribution to the economy a reappraisal of the mechanisms would be needed.

(4) At the same time private industry read the significance of the new domestic proiorities as they took shape in new legislation. Environmental pollution, product safety, occupational health and safety, auto safety, fire safety—all would call for technical solutions that were not in hand. The magnitude of external costs and benefits in commercial technologies was rapidly growing. Clearly a partnership between public and private sectors would be needed if all these problems and opportunities were to be met in a timely fashion.

(5) To cap it all, the economy was in a recession.

Let me grossly oversimplify the policy debate by characterizing Mr. Magruder's position as "monopsonistic" and that of a number of others (including myself) as "environmentalist." (A monopsonistic market is one with only one customer.) The monopsonists would have the federal government enter the market place directly as the first customer for major engineering development projects intended to meet the needs of the domestic commercial market as perceived by the government. Immediate employment for engineers and business for industry would result. Once the product embodying the new technology was brought into being, it was assumed that private customers would step forward. Aerospace companies, which are structured for this kind of contract work with the government, would probably get most of the business. It would be hoped that their easy entry into the new technologies would assist them in developing a profitable line of commercial business, meeting civil needs. Bold numbers like a $1 billion increment during the fiscal year 1973 to the federal R&D budget could be heard in Executive Office Building corridors.

The environmentalists, on the other hand, were skeptical of the ability of government agencies to read the commercial market and generally believed that if industrial R&D seemed deficient, it was probably because of obstacles to its profitable use, rather than a shortage of capital or talent to carry it out. We argued that government's responsibility was to ensure that the environment within which the private sector seeks to operate profitably should contain a minimum of barriers to the profitable application of research, consistent with the achievement of social goals embodied in congressionally established restraints.

Included in this "environment" is the pool of publicly available technical knowledge (which includes the body of potentially applicable technology) and the manpower resources of the educational system. Thus the environmental approach is by no means free from the need for public investment; the investment, however, would be aimed at strengthening the scientific and technical infrastructure of the economy and at satisfying the government's own new-product needs imaginatively, rather than being focused on contracting for particular products or with particular companies. The identification and removal of barriers to innovation would call for an industry-by-industry analysis, since each situation is highly unique.

The fiscal 1973 budget request to Congress and the accompanying Presidential messages revealed the outcome of the debate: a compromise embodying much of the environmentalists' philosophy but

still containing a significant expansion in government contracts for technology demonstrations in various fields where the governmental agencies had operating authority. The new policy toward expanded federal involvement in commercial R&D was still obscure, for a cornerstone of the new budget was a $44 million request for an Experimental Technology Incentives Program (ETIP), whose purpose is to explore empirically the basis for such a policy. Contrary to the long tradition of Office of Management and Budget vigilance to root out duplication of programs between agencies, NBS and NSF were both chartered to develop essentially similar programs of experimental contract arrangements to encourage private investment in an effective use of R&D.

If one takes the launching of this program as evidence of the government's serious interest in the problem, the opportunity to evolve and demonstrate an economically effective and politically acceptable relationship between federally sponsored R&D and commercial businesses seems to be at hand. If these programs serve no other purpose than to stimulate discussion, they will have been of significant value. Unfortunately, neither program is at this time well defined or understood and the net effect of establishment of the NBS ETIP program has been to divert resources from research services aimed at improving the efficiency and technological capability of the civil R&D community.

An improving economy, a gradual but steady reduction in unemployment, and the passage of an election year may have lessened the sense of urgency in Washington to work out a policy that fits our unique American circumstances. But last year's short-term economic and political problems should not be allowed to mask long-term issues that sooner or later must be faced.

LONG-TERM NEED FOR TECHNOLOGICAL PROGRESS

(1) The gross mismatch between the deployment of our scientific resources and the priority needs of the society persists. The research community is highly motivated to tackle the hundreds of urgent pollution problems, to tackle the fire safey problem, to find new ways to conserve resources and generate power. The pool of research and development talent for strengthening the economy through innovation could be quickly expanded. The resouces don't seem to be there.

(2) The country's opportunity to increase the rate of improvement of productivity is most striking in the service sector of the economy, which now engages the efforts of almost two-thirds of the work force. Public services in particular have shown themselves resistant to increases in productivity partly because outputs are hard to measure and the incentive structure is weak. The manufacturing sector of the economy—which also must provide the positive margin in export trade—must therefore continue to improve efficiency and find new products and markets. More important, the distinction between products and service, as between hardware and software, is fast eroding. We must bring to services the productivity-enhancing power of advanced systems thinking, or else the laborwasting, error-prone characteristics of many traditional service operations will increasingly frustrate the usefulness of efficiently manufactured products.

(3) Despite recent cuts in private-sector R&D expenditures, American business needs technology more than ever; it is getting more expensive and requires more sophisticated management to use advantageously. Manufacturing is moving from piece parts assembly to process technology in industry after industry. Not only is the manufacturing process increasingly automated but so is the engineering design process. The manufacturing process is, consequently, heavily determined by product design and, indeed, the manufacturing line may also be used as a development line. Thus, product design can now be and must increasingly be tailored to particular user requirements. Turn-around time from a new product idea to first manufacture has been cut from many months to a few weeks in the electronics industry. Thus market planning, product design and development, and manufacturing are increasingly tied together through rapidly evolving technology. As manufacturing costs are increasingly shifting back to the development and design phase, engineering—hard and soft—becomes an increasingly important but increasingly expensive business element. Such trends have had little or no impact on the federally sponsored programs that largely determine the character of the science and technology infrastructure of the country.

Thus the teaching of "design" in our engineering schools has all but vanished, being considered a fancy name for engineering drawing, while in advanced industries design automation has become one of the most sophisticated, demanding, and important elements in the research, development, and manufacturing process. A yawning gap separates the notion of "research and development" as seen from the vantage point of universities, and the agencies that support them, from the reality of science and engineering as practiced in high-technology industries. This gap is paralleled by lack of awareness in most government circles of the areas of science and engineering where progress will be most important to the economy in the future and how much more available, reliable, and appropriate to applications needs the knowledge and talent base on which the economy rests could be than it is. We must stop ignoring the inefficiencies, waste, and obsolescence in the science and technology infrastructure of the economy.

(4) One route to productivity increase is the quest for economies of scale; the natural end point is the world market. For many high-technology products for which the development costs run to hundreds of millions of dollars, such as civil aircraft, a world market must share the burden of the development cost. But economic nationalism abroad and a rising specter of short-sighted protectionism here at home threaten the goal of a continued reduction in international trade barriers.

(5) Finally, if we are to achieve social goals, including full employment and a more equitable income distribution, by fostering economic growth rather than by "robbing Peter to pay Paul," the economy must be structured to take advantage of U.S. competitive strengths: our technology, our educational levels, and the available capital.

There seems ample motivation, then, to address the responsibilities of the public and private sectors with respect to the commercial technology. First let me dispatch two points of view that continue to obscure the real issues. The first misconception is the notion popular in

technical circles, that the objective of public policy should be more research in industry. Let me quote Patrick Haggerty (2), whose company, Texas Instruments, has grown by 4,000 percent since 194? largely through the successful commercial application of very sophisticated technology derived from contemporary physics:

A technologically based company exists to create, make and market useful products and services to satisfy the needs of its customers throughout the world. It does not exist to do research and development or to provide jobs for scientists and engineers, for skilled craftsman or for unskilled laborers. It does not exist to provide careers for upper management. All of these, independently are useful contributions to society for that reason, society frequently legislates with respect to them but in the process, sometimes to its own detriment, acts as though one or the other of these were the primary business function. Further, it does not exist to provide a profit for its shareholders, although that may have been a primary motivation of the founding of the organization. However, the opportunity to make a profit is a company's incentive to create, make and market usable products and services.

The reasons these truisms must be emphasized is that research is much easier to do well than it is to apply profitably. Industrial investments are limited not by the technical capabilities of the laboratories, but by the ease with which the results of research can be put to profitable use. Thus we are to see an expansion of research investments in industry it must come about through an improved environment for using research. It will then come about simply because business improves and more income is available.

A second common fallacy is that industry should be expected to contribute the major part of the resources needed to sustain the country's basic research. There is no doubt that some industrial laboratories have made very important contributions to knowledge. In large high-technology companies such laboratories have an essential role, and their contributions to the scientific and engineering community are important for the advance of the whole industry. In fields of special interest, such as semiconductor physics, such laboratories can achieve positions of intellectual leadership. But simple arithmetic will show that only the universities and national laboratories can cover the fields of science and technology comprehensively.

Consider a company that spends 10 percent of its earnings on research and development, of which 10 percent is for pure and applied research, the remainder going to product and process development. If 20 percent of this research budget is for basic science—that is, following lines of inquiry in selected fields, guided primarily by maximum intellectual interest—you can quickly see that a company with gross sales of $1 billion could support basic research manpower equivalent to one university department. There are 127 manufacturers that big in the U.S., but there are over 150 universities granting science and engineering Ph.D.'s, each with many departments, and in addition, national laboratories with major resources. Thus industry should be able to make a major contribution to new knowledge but will never be its predominant sponsor. Industrial investments in science fall far short of the rule of thumb I have used, perhaps because these basic research investments must rest on management judgment rather than economic analysis.

We must not conclude, of course, that American industry uniformly undervalues the return on investment on research. Indeed, if you follow the old adage "Put your money where your mouth is," you would conclude that U.S. industry had more confidence in the value of research than did the federal government during the period from 1966 to 1971. Haggerty points out that "the total R&D performed in industry grew from * * * $10.5 billion in 1960 to over $18 billion in 1971. But since 1968, federal funds for industrial R&D have declined from $8.6 billion to an estimaed $7.8 billion in 1971. The increase of $2.7 billion (17 percent) in total industrial R&D since 1966 has come almost entirely from industry." Haggerty quotes NSF figures to say that industry's own investment in R&D rose from $4.4 billion in 1960 to $7.2 billion in 1966 to $10.5 billion in 1971.

It is foolish to suppose that the financial problems of universities will be solved by industrial funding, or that industrial research investments will necessarily rise as a result of access to government-sponsored research. Whether government funds add to or substract from private investment depends critically on how close to the long-term commercial interests of the company the government's interests lie.

For its own long-term survival the industrial community will certainly have to ensure an adequate supply of new knowledge and new ideas, produced and communicated in ways that permit them to be put to practical and profitable use. This must be done in part by direct investment in industrial research and in larger measure by industrial support for sound public policies for investment of tax revenues to support research, education, and more effective diffusion of scientific information worldwide. Nonetheless, if we are to understand the factors that determine the level of research opportunities in industry, we must recognize that R&D must be viewed as a means to an end (innovation) and not as an end itself.

THE TECHNOLOGY DELIVERY SYSTEM

Let me try to clarify the distinction between innovation and R&D. The oft-repeated suggestion that the federal government should sponsor private R&D as a means of stimulating innovation is a product of this confusion. While a necessary requirement for innovation embodying new technology, R&D does not and cannot cause innovation. Innovation—the introduction of new products, processes, or services to the market place—often requires no R&D at all, as Hollomon has pointed out [3]. Very few firms engaged in research; most of those who develop new products do so through the engineering design process. Design may require much inventive talent and it can lead to very profitable new products, but it does not advance the basic engineering state-of-the-art.

Thus the innovation process, which Gellman has referred to as the "technology delivery system" [4], is concerned not with the creation of new technology but with the transformation of technology into products and services. My basic point is that the innovative process is driven by the market place, and the best the government can do is to minimize the barriers that impede the technology delivery system.

On the other hand, the state of the nation's technology is a matter of direct federal concern, for it is the infrastructure of knowledge,

talent, and experience out of which designers of products and services can produce innovations. Technology can be pushed by imaginative government investment in the right kind of basic and applied research. Innovations must be pulled from available technology.

Too many otherwise worthwhile government programs may be characterized as "pushing the noodle of technology through the sieve of market needs." Let me give you an example. When HUD's Project Break-through was launched, amid much fanfare that it would produce a *breakthrough* in the traditional technologies of the building industry and introduce new "aerospace-type" technologies, it was very skeptically received by people who knew the industry. Innovative materials, construction methods, and designs abound. The reason they were so little used does not derive from the fact that the government had not demonstrated them in a highly visible way. The problems are institutional, relating to the structure of the industry, its suppliers, and its market.

But HUD needed help in purchasing and evaluating the Breakthrough projects and engaged in NBS Building Research Center for the purpose. NBS noted that there were no accepted, performance-based criteria by which to make the evaluation or write procurement specifications for industrialized, modular housing. Thus HUD and NBS transformed the project into one of major importance to the evolution of building technology in the U.S. by using Project Breakthrough as a test bed for the generation of prototype performance specifications for industrialized building. The results of the NBS work have catalyzed national attention to the need for improvements in the building regulatory system and the feasibility of the performance approach, and have contributed important new technical knowledge to that end. New institutions, designed to produce more unity in the building standards communities and to take advantage of the performance-based standards, have come into being, and others have been proposed. Many elements of the building community are not fully content with the specifics of the rapid institutional change that is now under way. But progress in that sphere can be made in no other way. In this form the Breakthrough program can be regarded as an excellent illustration of the intended purpose of the Experiment Technology Incentives Program.

In this example, please note that, viewed in this light, the Breakthrough project does not stand or fall on the market acceptability of the demonstration dwellings under construction. A nationally coordinated system of performance-based building standards would be a magnificent accomplishment; it is unnecessary that the managers of Breakthrough attempt to be as expert on consumer preferences and cost sensitivities as a successful speculative builder must be.

In raising doubts about proposed federal programs of direct support to commercial research and development. I do not want to leave the impression that federal procurement of development is never an effective stimulus to commercial development. Indeed, the computer field is an excellent example, for the needs of government during and after World War II were not only urgent enough to justify high-risk investment but the requirements were not too dissimilar from the needs of the civil market. That civilian market long ago outstripped the government's demand, which NBS tells me is about 14 percent of the

data-processing business. A similar history lies behind America's strong position in civil aviation. It is a very fortunate, but unhappily not very common, circumstance when the government's high-priority needs, justifying development of new-product technologies, are analogous to unmet needs of the commercial market. Even when the functional character of the requirements are similar (as in military and general aviation radar), the price and performance relationships are often so different as to call for different technological approaches.

TECHNOLOGY AND MARKET DEVELOPMENT

A great many people who exert strong influence in the field of public policy relating to R&D are inadequately sensitive to the dynamic character of the interrelationship between technology and market development. The fallacy of the monolithic market comes easy to those technologists whose accustomed image of a market is a clearly stated long-term requirement of the government, such as a rocket motor of a given specific impulse. Any understanding of the factors influencing industrial R&D investments must take into account the dual hazard of technological and market risk. Even in industry, there is often a tendency to regard "the market" as an implacable array of established public needs and associated commercial values.

But the market place does not exist independent of the array of technological choices out of which the promise of a better life can be constructed by imaginative entrepreneurs. Really new technologies tend to create their own new markets; unfortunately these markets are hard to predict since they depend on the imagination and predilection of customers, who indeed are the most important participants in the market-development process. Even dramatic new technologies such as xerography and polaroid photography, which ultimately created their own markets met initial resistance from market-forecasters, who could not see beyond already established needs.

In thinking about ways to improve the effectiveness of the R&D process in industry, it is a mistake to assume that only science and engineering are involved. The process of innovation calls for the development of new markets as well as of new products. I believe that creation of demand for a function that was never served before should be looked upon as an R&D process not unlike the process for new-product development. Traditionally businesses have expanded by bringing out better products for their accustomed market. The manufacturer or the customer may also discover advantageous new ways to use the new product, leading to the expansion of market opportunity. Or a new use for an old product may lead to the recognition of better technologies to satisfy the new need more profitably. Both processes lead indirectly to new business: better ways to do new things. It takes a very sophisticated organization to develop markets and technology simultaneously; it cannot be done without intimate knowledge, and even the cooperation, of the ultimate customers.

The similarity of the market and product development processes can be illustrated in the following listing of the elements of new market development:

Analysis to identify needs now unsatisfied
Pilot testing of new functions, in cooperation with users

Measurement of user's added value of displaced cost
Evaluation of institutional barriers, secondary risks
Selection among alternatives of innovations with best potential
Development of complete marketing and service resources

I do not suggest that government agencies can easily assist directly in the process of bringing to commercial maturity the needs of the society in such a form that the economic justification for new technology becomes self-evident. Nevertheless, because of the government's poor qualification as a predictor of commercial markets and the generally unhappy prospect of government scientists having to answer the question "Who needs it?" after a technically exciting project has been finished, there may be merit in suggesting that the government's most propitious partnership with the "technology delivery system" is with the users, not the generators, of technology. Assistance to the generators of technology should come through investments in the scientific and technical infrastructure.

Last year I heard about a very interesting new invention that uses superconducting magnetics of prodigious field strength to separate out difference sizes of particles in a slurry. It was suggested that this could be uesd profitably in extraction of kaolin from clay for use in making specialty papers. Unfortunately, the clay pits of Georgia do not seem to be a very good place to persuade laborers to work with large quantities of liquid helium and to remember to leave the steel crescent wrenches out of their overalls. In such a case, it would make much more sense for the government to join with the clay diggers (preferably all of them, through their trade association) to evaluate the idea, fund the needed adaptive development, and give it a suitable field trial. The intended users of the new technology are much more likely to keep realistic cost discipline, to face the practical questions of training and operations, and to be alert to alternative cheaper solutions to their problem than are the developers of the technology.

The British demonstrated the value of this principle very well in connection with numerically controlled machine tools. A technologically advanced firm was making excellent machines, but was in trouble because of resistance by British manufacturers to the risks of using something new and unfamiliar. The government purchased a number of the machines placing them in factories at no cost. The *quid pro quo* was that factories getting the new machines were required to use them in their regular production lines and to provide the government with a detailed report on their economic and technical performance. At the end of the year the users were permitted to buy the machines from the government, and most did, for they found them very productive. Thus the oldest barrier of all—the fear of something new, however promising—was successfully addressed, and the manufacturer achieved an adequate initial market acceptance for further growth.

By now my message should be clear. Commercial innovations will happen when the environment is right. But the simple existence of a product idea, while necessary for commercial success, is far from sufficient. The most dramatic new business successes sometimes—maybe usually—results from a new answer to a question no one was clever enough to ask. But these success stories don't happen if the new product idea never reaches the market in the first place.

Thus the focus of any policy on federal encouragement to commercial innovation, and therefore to commercially relevant R&D, must be on removing the disincentives and barriers to market demand for new products and services as well as the barriers to technological success. I have discussed two barriers to technological innovation: (1) reluctance of management or customers to accept unfamiliar technologies that require changes in accustomed ways of doing business and (2) uncertainties and difficulties impeding the development of new markets for new products. Acceptance of change entails risk—risk to the manufacturer in the first instance and risk to the customer in the second. But in addition to barriers that are inherent in the process of change itself, there are many that are institutional in character. Most of them are market, not technology, barriers. Some of them are:

(1) Design-based regulatory restraints on new products and processes, which suppress superior technical solutions to the problems. Examples abound in the building regulatory system, in transportation, pollution, and safety regulation.

(2) Fragmented markets, such as typically prevail in public services due to dominance of noneconomic factors in purchase decisions and inability of local governmental units to evaluate alternative products objectively. Examples are found in fire and police services.

(3) Incompatibility of the organizational structure of the market with easy accommodation of an innovation (such as containerized freight) that may shift market share among strong economic interests. Such barriers are particularly hostile to systems optimization of industries not yet vertically integrated.

(4) Nontariff barriers to trade, such as incompatible standards or requirements for certification to standards in the market area. Our archaic inch-based measurement system is an example of a self-imposed nontariff barrier, which will become progressively more troublesome with passage of time.

(5) Failure to resolve blockages in domestic standardization (which, for example, has impeded the introduction of plastic plumbing pipe and quality grading of lumber); or premature introduction of standards (which are valuable only when their contribution to overall economic efficiency and growth outweighs their tendency to inhibit further innovation). In special cases, standardization is a prerequisite to initial introduction of a useful technology (as in color television or freight car logistics), making it difficult for market forces to select the best technological solution for implementation.

(6) Uncertainty in the future course of regulatory requirements, both because of changing scientific evidence regarding hazards and changing political consensus on the level of control required for public protection, which can delay investment in a technology whose costs must be amortized over many years.

Other barriers are associated with the inflexibility of the work force and traditional suspicions of technology to enhance productivity. But the experience of the last few years' discussions on the productivity issue suggests that the trend toward increased job security is bringing with it a much greater acceptance of new technology that ultimately contributes to better pay and working conditions.

These are examples of institutional and market barriers; each has some technical content, but none can be dealt with by research and development alone. And none can be overcome by a program of broad-gauged federal subsidies or preferential tax incentives for industrial R&D. In virtually every case, the mitigation of such barriers requires institutional change. Since addressing institutional issues is a natural role for government, it may seem surprising that enthusiasm for a policy directed at removing institutional barriers to innovation is not universally applauded. The answer is very simple. Such programs, if effective, are politically controversial. It is much safer to advocate the appropriation of vast sums for the purchase of technology that the government may or may not need. The agency enjoys running the program, the vendor makes a high and virtually risk-free return on investment—even if only a modest profit margin—and the congressman from his district will defend the public value of the expenditure.

If one looks for the source of opposition to the Hollomon Civilian Industrial Technology program of a decade ago, it can be found in the building materials industry, which feared a federally induced perturbation in the product market caused by the introduction of both new technologies and new systems organization in the building process. Those concerns still trouble the industry, but—as I mentioned in my discussion of Project Breakthrough—great progress has been made. A recent example is the establishment, through NBS encouragement, of a national organization of state building regulatory officials (the National Conference of States on Building Codes and Standards) primarily for the purpose of addressing the program of industrialized modular construction. NBS has fostered and cooperated with many other private organizations whose existence has helped improve the market place for new technology.

MEASURING EXTERNAL BENEFITS

In addition to all the market-related barriers I have discussed there is one more which is the most often cited in economic discussions of the question: Does commercial industry underinvest in R&D, and if so, when and why? I put this question late in my discussion, because I believe that virtually everyone would agree that a free competitive market place is a theoretical ideal, to be sought but never fully realized. There are many constraints and rigidities that distort the optimum economic decisions that might otherwise describe the economic equilibrium.

The contemporary economic argument is that, even in the absence of barriers or disfunctions in the economy, individual entrepreneurs will invest less in R&D than would be justified if one weighs private costs against benefits to the economy as a whole. In other words, the R&D investor pays the whole bill but shares the benefits with his customer (if his price allows the customer a productivity increase), with his competitors (when the technology appears in the market place), and perhaps with the society as a whole (if the product produces an indivisible public good such as improved public safety). We could all make up examples of products that differ in the extent of the values external to the initial transaction. The added public benefit from the invention of a machine tool that is 100 times more productive and also safer to

use than its predecessors is surely greater than that from a new fashion in cosmetics. Similarly, there are markets in which advertising plays a great role in consumer choices, where R&D is devoted to product differentiation of relatively low social utility. Granted that there are such differences, what should the government do about it?

First, it is clear that research is needed to find econometric methods for quantitative estimates of benefits external to those captured by the technology vendor. This could well be a main thrust of the ETIP program. If such methods existed, many programs of federal assistance to labor and business, such as the loan guarantees of the Small Business Administration to venture capital organizations, could be designed to give preference to ventures with maximum public benefits. In the absence of a quantitative economic tool, such efforts would be swamped in political controversy.

The government could give priority to technologies judged through political consensus to have large external benefits by addressing the market barriers to innovation in these areas with special vigor. This, indeed, is what happens now, and explains why NBS efforts focus on housing, pollution, fire and product safety, structural failure avoidance, clinical chemistry, and information technologies.

Finally, the long-term goal should be to find ways to internalize as many of the external benefits as possible so that market incentives will provide the appropriate incentive to R&D. I can illustrate this hypothetically and facetiously by suggesting a law that all automobile engine exhausts must be discharged into the passenger compartment. Then the owner would have exclusive benefit (and cost) of the pollution, and he would soon either take up walking or pay what it takes for an automotive technology that produces nice-smelling exhaust of no toxicity or no exhaust at all, such as an electric car. No tax money would be involved in the transaction. Note that my example also illustrates what I mean by a performance standard: the choice of best solution to the problem is left to the R&D market place. Henry Ford III was quoted in the press as suggesting (perhaps more seriously) that water pollution be addressed in a similar manner: let every factory's intake pipe be located downstream from its outflow.

I wish the economists good luck in their search for ways to make Friedmanism work by internalizing all external social costs and benefits. Some brilliant and persuasive members of this clan are at work in a task force of the AAAS Committee on Industry Technology, and Society. In the measure of their success will be the reward of natural incentives for allocating private R&D investment according to need. In the meantime, I suggest a simpler, safer, and more easily implemented policy.

Let us use the democratic process to identify the indivisible public costs and benefits—that is, the bad things we wish to avoid sharing and the good things we wish everyone to enjoy—and set priorities for two broad federal efforts. The first would identify and address barriers to useful innovations and in voluntary cooperation between government and the private sector set about mitigating them. Second, let us restructure the priorities within the federal science and technology policy to improve the efficiency, applicability and innovative power of the country's technical infrastructure. In particular let us

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focus more resources on generally useful, nonproprietary applied research services to the users of new scientific knowledge—services to advance the state of the art of commercial technologies.

I will say only a few words about the importance of the knowledge and skills environment for commercially relevant R&D. I touch briefly here only because I don't want you to leap to the wrong interpretation of my intent. I probably should have devoted a large share of this paper to deploring the baleful neglect that for fourteen years has surrounded the National Standard Reference Data System, probably the program least self-serving and most directly useful to industry in the whole federal government. The Weinberg Panel of the President's Science Advisory Committee saw a need for a $10–20 million budget a decade ago; the budget today is a scant $2 million. I could certainly have pointed out how extraordinary it is that our government spent tens of millions of dollars on the metallurgical problems of the Rover nuclear rocket engine no one ever had a use for, but we can't find a few hundred thousand dollars for a program to help industry with structural failure problems that take a steady toll in human life and are a drain on the economy. Our schools do not give our young scientists and engineers adequate training in error analysis and control, or train them in how to acquire, evaluate and communicate scientific and technical information. Few U.S. engineering schools teach product design; only one offers a Ph. D. in manufacturing engineering. And the government makes only marginal efforts to ensure that the results of our national research effort, largely supported on public funds, are made effectively available to those who would use the information.

The National Science Foundation has excellent, well-funded programs in basic research and some interesting experiments in socially oriented Research Applied to National Needs. The RANN program emphasizes multidisciplinary studies, including a requirement for inclusion of social science aspects. Many problems require this approach, but it is questionable whether this approach does much for the general level of R&D productivity in industry. It is much more difficult for professors to find NSF support for systematic reliable research on the characterization of polymers on cutting-tool wear, on the mechanisms for the fracture of glass or the suppression of combustion, or on the systematic measurement of cross sections for specific energy transfer processes in useful materials. In short, little of the NSF program is aimed at advancing commercially relevant technologies. Meanwhile, NBS, which conducts in-house studies in such fields and has countless technical contacts with industry, has no funds with which to extend participation in the effort to universities or industry. And NSF finds collaboration with NBS strangely difficult.

If I had given this set of concerns its due, you might have concluded that I believe that balancing our federally sponsored research investments and giving adequate support to knowledge needed by users in industry is the answer to the question posed in this paper's title. It is not. The knowledge and talent base in only a necessary, not a sufficient, condition for economic success. One need only look at Britain today and Japan fifteen years ago to see both sides of that lesson. But we would do well to remember that the "state of the art" within which

engineers work is no more static than the market place to which their products are sold. While we are improving the market environment for R&D, we must also be increasing the efficiency of the commercial R&D community very substantially as well.

You can see that I find myself uncomfortable with the ETIP program as many conceive it—that the way to approach the identification and removal of barriers to innovation is through contractual relationships with single companies for the conduct of commercial research and development. It is hard to imagine that decisive results will flow from government-funded development unless the government is the *real* rather than the *surrogate* customer for the resulting product. We have no way to find that narrow band of return-on-investment that is both insufficient to justify investment of private capital yet somehow sufficient to justify the government's participation. Furthermore, the political problem attaching to the selection of a particular company in the absence of an objective criterion is worrisome. I conclude that the most valuable contribution the government can make to commercial companies is probably not in the form of *direct* financial assistance at all.

Indeed, I feel sure that the economic leverage of programs aimed at improving the environment for innovation in building, setting performance specifications for fire and police technologies, providing tools for quality control in clinical chemistry, and generating test methods for materials failure avoidance, just to name a few, far outweigh the value of most of the unsolicited proposals for ETIP received when I was still at NBS. After the excellent policy basis set in the President's R&D message to Congress in March 1972, new funds were appropriated to NBS for many well-thought-out programs that address barriers to private R&D without interfering with the free play of competitive forces. These funds have apparently been impounded. Out of the largest budget received in its history, NBS is at this writing only permitted to initiate one program: ETIP. If this situation is not remedied it will make a sham out of the President's first R&D message to Congress in history. It is very disappointing to see a pattern of successful work in support of economic development and public protection, which was welcomed by the industrial R&D community, turned off and replaced by a speculative program whose basis for usefulness is still to be established.

In summary, I feel that the federal investment in new research knowledge is not well balanced from the economic view, and much must be done to improve the usefulness of the knowledge environment on which all companies must draw. The industrial R&D community is capable of responding very quickly to new investments in commercially relevant R&D; the quality and efficiency of its response depends on the state of the art of the technologies on which industry draws. The stimulation of those new investments depends more on understanding and removing barriers to private incentive to innovate—on improving the technology delivery system—than on the availability of federal financial incentives to commercial research and development. The efficacy of federal programs that only subsidize the ongoing R&D effort can be tested by awarding, then removing, the subsidy. How much of the increment of effort will remain?

On the other hand, voluntary cooperative work between technically competent federal agencies and private organizations can improve

institutional and market structures without jeopardizing the play of free competitive forces A resurgence of innovation in commercial markets could result. We must begin with a sound science and technology policy for the nation, capable of reflecting both cultural and utilitarian values and implemented by a new institutional arrangement in which NBS and NSF can work in much closer and more harmonious collaboration toward the goal of releasing the country's inventive and entrepreneurial talents to the solution of the needs of our society.

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"Research and Development in Industrial Growth"

By William N. Leonard

[From the *Journal of Political Economy,* March/April 1971, volume 79, p. 232]

Research intensity, measured by company R & D spending, relates significantly to growth rates of sales, assets, net income, and other variables of sixteen industries performing nearly all manufacturing activity. The relation appears two years after R & D spending and increases thereafter. Research intensity, measured by manpower ratios relates less effectively. When research intensity ratios include federal R & D funds, correlations with growth rates fall, usually below significance. By eliminating two industries receiving five-sixths of federal funds—aircraft and missiles and electrical equipment—significance emerges between federal R & D intensity and growth rates. Industrial growth appears slowed by excessive allocation of R & D resources to defense-space uses.

INTRODUCTION

For some time economists have been interested in the relation between technological change and economic growth. Solow's well-known paper segregated the technical factor and attributed to it rather than to growth in capital stock most of the long-run increase in per capita output in the United States (Solow 1957). Later studies introduced other variables explanatory of growth, especially changes in the quality of labor, which tended to reduce the portion of increase in national product ascribed to technological change. Although the three principal forms of investment—physical capital, education, and technical change—interact so that increases in one stimulate returns to and increases in the other, the most prominent role in the growth of per

capita output still belongs to technological advance (Nelson 1964, 1965*b*).

Investigators have found the linkage between technical inputs and industrial growth complex are difficult to compute. A few studies have shown a positive association between research intensity, measured usually by ratios of scientific personnel to total employment or by R & D expenditures/sales, and gains in productivity, profits, and sales (Minasian 1962; Comanor 1965; Murphy 1965; Grabowski 1966), but these studies have generally concerned limited numbers of companies or a few industries, sometimes only firms making chemicals and drugs. Where investigations have been made of all manufacturing industries (Terleckyj 1960; Gruber, Mehta, and Vernon 1967; Keesing 1967), serious problems of data, methodology, or causality have arisen.

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SUMMARY

A study of sixteen industry groups performing 97 percent of all industrial research and development, and accounting for 92–95 percent of sales, assets, net income, and net worth of all manufacturing companies, provided a strong relation between research intensity measured by company R&D funds as a percentage of sales and the rate of growth of sales, assets, net income, net worth, and net plant, property, and equipment. The effect of R & D upon growth begins on the average in the second year after the R & D investment and continues with steadily rising influence for at least nine years after the initial input year, reflecting the rising proportion of sales consisting of new products developed through R & D. This index of research intensity was also correlated significantly with the rate of growth of real output (Federal Reserve Board index of production), of value added (Census Bureau), and of productivity (Bureau of Labor Statistics), although the latter data are based upon an "establishments" rather than "consolidated" basis of reporting. Correlations of other indices of research intensity, for example, total R & D spending/net sales, and scientists and engineers per 1,000 employees, with the various measures of industrial growth were generally lower.

An alternative hypothesis that causality ran from growth of industrial output to research intensity measured by company funds or manpower could not be sustained. A multiple regression analysis of the sixteen industries, using growth of real output as the dependent variable and independent variables of research intensity, increase in capital stock, rise in manhours worked, and educational attainment of workers—after eliminating one of the variables for collinearity—proved significant ($R^2=.70$). The analysis confirmed the significant influence of R & D intensity, measured by company funds or company-financed scientists and engineers, upon the rate of growth of real output. One other variable, the increase in manhours worked, had a significant influence in the equation.

All tests indicate intensity ratios using company R & D fund/sales or company-financed R & D personnel/1,000 employees to be more closely associated with rates of industrial expansion than ratios which included also federal R & D spending or federally financed engineers and scientists. However, by dropping out two industries which re-

ceived five-sixths of R & D funds—aircraft and missiles and electrical equipment, including communications—and including only the fourteen industries considered commercially oriented (in which federal funds are less than half of R & D expenditures), a strong positive association appeared between federal R & D spending (and scientific personnel supported by federal funds) and measures of industrial growth. The results confirm the thesis that the existing concentration of federal R & D spending in two industries is unproductive of growth, either because of diminishing returns to R & D encountered in these industries, or the failure of firms in these industries to realize the commercial potential of innovations arrived at in federally funded R & D programs, waste of resources, or a combination of these factors. The findings support Richard Nelson's hypothesis that the opportunity cost of federal R & D contracts in defense-space programs has been slower growth, reduced productivity, and lower quality of output in the civilian sector. It appears reasonable to conclude that either a free market for scientists and engineers or a redirection of federal R & D support to industries (manufacturing or nonmanufacturing) lagging in growth or technical change would better promote quantity and quality of output in the civilian economy.

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"The Relationship Between Interstate Variations in the Growth or R&D and Economic Activity"

By Ira Horowitz

[From *Engineering Management,* September 1967, volume EM–14, p. 135]

Abstract.—The relationship between regional R&D growth and subsequent regional economic growth is analyzed empirically. The rates of growth from 1920 to 1964 are calculated for various economic variables and R&D in the 48 contiguous states. The correlation coefficients of the growth equations measure consistency in growth. These

coefficients are then analyzed via a correlation procedure. The results suggest that regions enjoying the most consistent rates of growth in R&D activity will subsequently enjoy, if not the most rapid economic growth, the smoothest and most consistent pattern of future economic growth.

INTRODUCTION

Implicit in the concern with interregional disparities in R&D effort is a presumption that firms will tend to locate production facilities to exploit R&D in the originating regions. Hence, one might suspect that unique economic benefits will accrue to regions primarily responsible for R&D. Still, regional economies are open and the facilities to manufacture a product developed in one region of a nation may well be established elsewhere. This paper analyzes empirically the relationship between regional R&D growth and subsequent regional economic growth. The results suggest that regions enjoying the greater rates of growth in R&D effort subsequently enjoy singular economic benefits, and further, that the major benefit has not necessarily been *higher rates* of economic growth, but rather a *more consistent rate of economic growth*

THE UNDERLYING MODEL

Our major purpose is neither to detail a theory of the role of R&D in the growth, survival, and prosperity of the firm, nor to set out a theory of the role of local firms in regional economic growth. Rather, we posit only the elements of such a theory, set out the framework within which the firm undertakes R&D, and infer the potential consequences. These ideas form the basis for the argument that regional R&D activity has implications for regional economic growth.

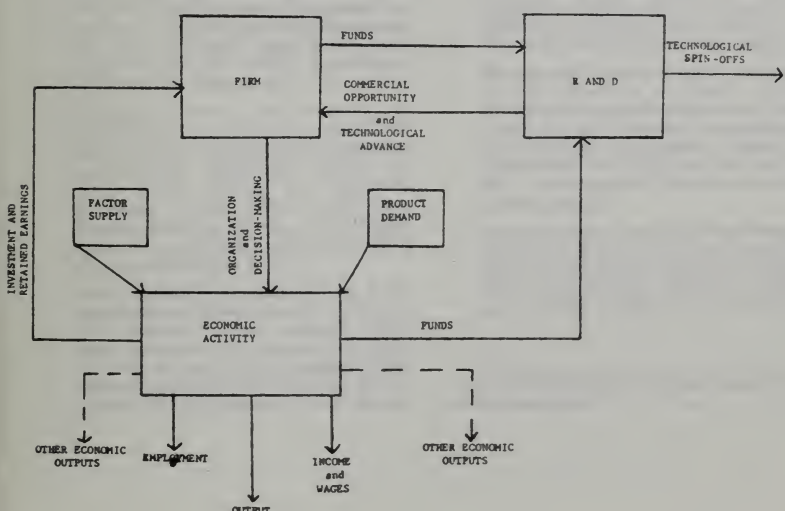
Fig. 1 presents what is, perhaps, an overly simplified model of a firm's operations. Here organization and decision-making activities act as catalysts upon the factors of production in satisfying product demand and generating economic activity. Employment, income, output, raw material purchases, and the establishment of distribution channels are typical of the affected variables. Similarly, profits may be retained, distributed as dividends, or invested to replace existing equipment or expand existing facilities. The function of R&D in this model can be viewed as providing the additional commercial opportunities and technological advances to make the firm a more effective producer. The firm, in turn, provides the funds to finance R&D, and these funds may come, in part, out of the profits generated by production.

If firms and economies were structured as simply as depicted in Fig. 1, it would follow that increases in R&D, presuming the R&D was effective, would stimulate aggregate economic activity, barring the possibility that others lost competitive position and thereby suffered losses in excess of the gains made by the R&D oriented firms. Nevertheless, not all R&D has a direct (or indirect) economic payoff; where there is a payoff, it will not necessarily accrue to the originator; and where the payoff does accrue to the originator, the firm will not necessarily choose to exploit the results in the region where the R&D

was undertaken. Clearly, then, even if regional R&D and *subsequent* economic activity were related, the relationship could be expected to be neither very precise nor very perfect. Still, not all firms conducting R&D manufacture in several regions. Further, any tendencies toward spatial decentralization will make it more likely that a new commercial venture or technical advance will indeed be exploited in the originating region. Still further, if there are technological spin-offs from R&D, knowledge of these may more readily be acquired by firms enjoying spatial proximity to the originator. In essence, then, it is suggested that when an Indiana firm embarks on an effective research program, the firm's subsequent economic benefits, and the economic activity that is generated, are *more likely* to be felt in Indiana than in Iowa.

These arguments assume that the payoff from a firm's R&D effort will not be offset by compensating encroachment upon others. It is hypothesized that these effects will in part carry over to the regions housing the originating firms. In particular, two additional hypotheses are suggested: 1) the higher the rate of growth in a firm's R&D effort, the greater and smoother will be its *rate* of economic growth; and 2) the *smoother* the flow or R&D activity, e.g., the more consistently it is carried out over time, the greater and more consistent will be the firm's economic growth. The hypotheses to be explored refer to whether the latter effects will in turn carry over to the regions in which R&D originates. If the preceding arguments hold, we should note a relationship between growth and consistency in the rate of growth of R&D activity in period T, and growth and consistency in the rate of economic growth in some later period $T+k$.

FIGURE 1



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SUMMARY

This paper has attempted to provide evidence in support of the hypothesis that there is an economic payoff to R&D. In particular, the analysis undertaken has sought to establish this relationship at the regional level, and to show that regional R&D effort is subsequently reflected in regional economic growth. Further, the results suggest that regions enjoying the most consistent rates of growth in R&D activity will subsequently enjoy, if not the most rapid economic growth, the smoothest and most consistent pattern of future economic growth.

"Industrial R&D and Innovation" (National Science Board)
From *Science Indicators 1974*, p. 81

Industrial R&D and Innovation

INDICATOR HIGHLIGHTS

- Total expenditures for industrial R&D more than doubled between 1960-74, with one-third of the growth occurring after 1971; the large increases in recent years came almost entirely from industry's own funds, raising the total expenditures for industrial R&D to more than $22 billion in current dollars in 1974.
- Adjusted for inflation, total expenditures in 1967 constant dollars for industrial R&D were $15.2 billion in 1974, which was 11 percent lower than in 1968-69, the years of highest funding, and approximately equivalent to the funding level of 1965; in 1974, development activities accounted for 79 percent of total industrial R&D expenditures, compared with 18 percent for applied research and 3 percent for basic research.
- The total number of scientists and engineers engaged in industrial R&D increased in 1973 and 1974 to 360,600, following a decline from a peak employment level in 1969 of 387,000; such personnel supported by industry's own funds increased throughout the 1960-74 period, while the number supported by the Federal Government declined to pre-1960 levels.
- Expenditures for applied research and development in industry are focused on six product areas: communications equipment and electronic components, aircraft and parts, guided missiles and spacecraft, machinery, motor vehicles and other transportation equipment, and chemicals; these areas comprised nearly 70 percent of all such expenditures in 1973.
- Industrial R&D is concentrated in a few manufacturing industries and in a relatively small number of large companies within those industries; five industries accounted for some 80 percent of all industrial R&D expenditures in 1973 and a similar proportion of all R&D personnel, while the 100 companies with the largest R&D programs spent nearly 80 percent of all industrial R&D funds.
- Improvement of existing products was the reported goal of one-half of all industrial R&D in 1974, compared with approximately 35 percent for developing new products, and 15 percent for new processes.
- The R&D intensity[1] of manufacturing industries declined steadily after 1964 as a result of reduced Federal support for industrial R&D (primarily in the aircraft and missiles industry); in terms of industry support alone, however, the level of R&D intensiveness has changed little since the early 1960's.
- The most R&D-intensive industries were the largest producers of patented inventions, accounting for over 67 percent of all patents granted during the 1963-73 period; the majority of patents were for inventions in six major product fields: machinery, fabricated metals, electrical equipment, chemicals, professional and scientific instruments, and communications equipment.
- The most R&D-intensive industries produced the majority of a sample of major technological innovations during the 1953-73 period; these industries accounted for 66 percent of the innovations, followed by intermediate level industries with 24 percent, and the least R&D-intensive industries with 10 percent.
- Large companies (those with 10,000 or more employees) produced a greater number of the sample of innovations between 1953-73 than companies with less than 100 employees, but a smaller number than firms

[1] The proportion of net sales devoted to R&D and the number of R&D scientists and engineers relative to total company employment.

employing less than 1,000; small firms (those with less than 100 employees and those with 100-999 employees) produced more innovations per unit sales than larger firms throughout the period.

- The largest percentage of the sample of technological innovations produced during the 1953-73 period represented improvements in existing technology (41 percent), followed by those representing major technological advances (32 percent) and radical breakthroughs (27 percent); the fraction of radical innovations declined 50 percent between 1953-59 and 1967-73, while those rated as major technological advances increased proportionately.

- The most frequently cited sources of the underlying technology for the major innovations were research (applied and basic), followed by the transfer of technology from existing product lines of the innovating firm, licensing, and the purchase of technical "know-how" from other firms.

- Basic research was more often involved in product innovations characterized as radical breakthroughs (68 percent) than in those rated as major technological advances (48 percent) or improvements in existing technology (45 percent); applied research occurred with nearly equal frequency in all categories of the innovations studied.

Returns to Research and Development Expenditure in the Private Sector

By Zvi Griliches

Discussion Paper, Harvard Institute of Economic Research, September 1975.

1. INTRODUCTION

In late 1965, the Census Bureau and the Office of Manpower Studies of the National Science Foundation asked me to consider a project to analyze the available historical data on company research and development expenditures together with other data for the same companies collected in different Census inquiries. During 1966–67, a plan of work was outlined, cut down to size, and agreed upon. The Census undertook to develop a company record, edited for consistency, to produce regressions and related outputs free of disclosures for individual companies, and to pass on the reasonableness of the various series employed. Only Cenus employees were to have (and have had) access to individual company data and the treatment of outliers was in accordance with the usual criteria employed by the Census. The process of matching the same companies in different data sets and over time turned out to be quite a difficult and time-consuming task. Because the results were slow in coming, and the in the context of severe budgetary cuts, the Office of Manpower Studies of the NSF bowed out as a direct partner in this study in 1968. The rest of the financing for this project still came from the National Science Foundation, but in the form of a direct research grant to me rather than as a continuation of the in-house research partnership. The funding crisis and other workload pressures on the Census delayed the completion of the data match until 1970. During this long gestation period the project was greatly reduced in scope by abandoning the idea of extending the match to such additional company data sources as the IRS and Compustat tapes and by limiting the number and range of variables to be included in the final data base. First regression results for a restricted set of equations and variables became available in early 1971 and final corrected runs were delivered in 1972. This is the first report based on the results of this project. I am solely responsible for the interpretation and analysis of the results and for the delay since mid-1972.

The original universe of this study consists of large (1000-plus employees) R & D performing U.S. manufacturing companies. There were 1,154 such companies in 1964. Our final sample is based on data for 883 such companies, accounting for about 90 percent of total sales and over 92 percent of total R & D expenditures of all firms in this universe (see Table 1 for more detail). Since large firms account for most of the reported R & D expenditures in industry, our sample accounted for 91 percent of all the R & D performed in industry in 1963 including the R & D performed outside our universe of large companies. Thus, in spite of quite a few companies for which some or much of the data are missing, the coverage of our sample is rather complete, especially in comparison to other microdata sets of this kind.

The data base consist of individual company time series on research and development expenditures (company financed and total), on the number of research scientists and engineers, and on total company employment and sales—all based on the 1957–65 annual NSF-Census R & D Surveys, and of additional company data on value added, assets, depreciation, and other economic magnitudes, based on the match with the 1958 and 1968 Census of Manufactures and Enterprise Statistics. Because of problems of handling confidential data I received only matrices of correlation coefficient and standard deviations for the variables in the data base, broken down into six rather broad industry groupings, and never had access to the actual individual observations. The restriction of this study to variables contained in the original data sets and the associated inability to add such things as prices, stock valualtion, or concentration ratios, the availability of the data only in the form of moment matrices, the relative shortness of the available time series, and the lack of detailed industrial breakdown, all limit severely the range of questions that can be asked and largely predetermine the feasible modes of analysis.

When this study was initiated in the mid-1960's, my own interests centered on sources of productivity growth and on estimating the contribution of non-market factors to growth using production function models and econometric estimation techniques. The study reported below bears the marks of this interest. It focuses on estimating the coefficient of cumulated R&D expenditures in company-level production functions or its equivalent in company productivity growth equations. Because the data are for individual companies, this study can explore only the magnitude of *private* returns to such expenditures. It cannot deal with the very important issue of externalities; returns that accrue to other firms and to the society at large and are not captured by the original investors. In a later paper I shall try to deal with this problem by comparing the estimates presented here with those derivable from aggregate industry and economy-wide time series. Here we'll limit ourselves, however, to what direct information can be gleaned from the data at hand.

* * * * * * *

VI. DISCUSSION AND SUGGESTIONS FOR FURTHER RESEARCH

In spite of various reservations, we have found a rather consistent positive relationship between various measures of company productivity and investments in research and development. In particular, Cobb-Douglas type production function estimates based on both levels (1963) and rates of growth (1957–65) indicate an overall elasticity of output with respect to R&D investments of about .07, which can be thought of as an average of .1 for the more R&D intensive industries such as Chemicals and .05 for the less intensive rest of the universe. These findings are consistent with the earlier findings of Mansfield and Minasian, but are based on a much larger and more recent data base.

It is rather hard to convert the estimated $a = .07$ into an estimate of the rate of return to R&D investments. Accepting our estimates, and the validity of our measures and using the elasticity formula to derive the implied marginal product estimate, yields .26 as the overall estimate

the average gross excess rate of return to R&D in 1963. This is an average for 1963 because it is based on a function fitted across all the firms in our sample and because it is evaluated at the average total cumulated R&D to value added ratio in 1963 in our sample ($K/V=.26$). It is "gross" because neither our measures of ouput or of input allow for any depreciation of past R&D investments and it is "excess" because the conventional labor and fixed capital measures already include the bulk of the current R&D expenditures once.

While our industry groupings differ in the estimated level of this elasticity, they also differ markedly in their R&D intensity, which actually results in much less difference in the estimated rates of return than one might have thought to start out with. Taking Tables 5 and 6 together, one might conclude that α is about .1 or higher for industries and 1 and 2, between .05 and .1 for industries 3, 4, and 5, and less than .05 for industry 6. Since the average K/V ratios for these industries are .23, .23, .6, .16, 1.4, and .09 respectively, the implied rates of return are approximately .43, .43, .08, .31, .04, and .44 respectively (taking α as .1 for industries 1 and 2, .05 for industries 3, 4, and 5, and .04 for industry 6). Thus, except for industries 3 and 5, the resulting estimates of the private rates for return to total R&D are on the order of 30 to 40 percent. These estimates are larger, but not inconsistent with those presented in Table 7, based on an entirely different dependent variable (GRR). There too, the two industries with the largest federal involvement in the financing of R&D (3. Electrical Equipment and 5. Aircraft and Missiles) yield the lowest rate of return estimates.

It is interesting to note that we have stumbled on this impact of federally financed R&D in the interpretation of our results rather than in the econometric analysis itself. In our regressions we were unable to discover any direct evidence of the superiority of company financed R&D as against federally financed R&D in affecting the growth in productivity. It may well be the case that within any company a dollar is a dollar, irrespective of the source of financing, but that in these two specific industries the externalities created by the large federally financed R&D investments and the constraints on the appropriability of the results of research that may have been associated with such investments, have driven down the realized private rate of return from R&D significantly below its prevailing rate in other industries.

In general, this paper can be viewed as another supporting link in a chain of a rather limited number of investigations for the argument that R&D investments have yielded a rather high rate of return in the recent past. In addition, we find no evidence for and some evidence against the notion that larger firms have either a higher propensity to invest in R&D or are more effective in deriving benefits from it.

There is little point in reiterating the various reservations outlined earlier. Some of the difficulties are inherent in the attempt to measure and discuss "research" and "productivity" as if they were clear and unequivocal concepts. But many of the problems, particularly those dealing with timing effects, spillovers, and externalities, could yield to more data and better data analysis. It would be very useful to have more detail on the firms at hand, especially information on the distribution of their research expenditures, on other measures of

research output such as patents granted and papers published, and on income received from royalties and money spent on advertising. All of this is feasible, it requires "only" the additional matching of IRS, SEC, and Patent Office and scientific abstracting services data bases. It would also help to know, for tracing out and following up potential externalities, more about the exact industrial structure of individual firms and their product mix. Finally, it should be relatively easy and quite useful to extend this study, as is, to the 1966–1974 period. Such an extension would be particularily interesting since it would allow us to observe a period during which R&D growth came largely to an end for many firms (at least in real terms). Besides helping us to find out something about the structure of lags and the rate of depreciation in such data, it would also, for the first time, break rather sharply the confounding colinearity between growth in R&D and the growth that occurred in almost all of the other economic variables during the 1956–65 period.

Even without new data, we have not exhausted yet what can be learned from the data at hand. Additional analysis of the data on the number of scientists and engineers as against R&D dollar totals should prove illuminating. The distinction between federally and company financed R&D has not been really explored in depth yet. Finally, a detailed comparison of the individual industry results with industry aggregates, focusing on the potential externalities (external to the firm but internal to the industry) is required before any strong conclusion could be drawn about *social* rates of return from our estimates of *private* rates of return to R&D.

"Research and Development and Productivity Change in the U.S., 1948–1968"

By John A. Shaw and Don R. Leet

From *The Journal of Industrial Economics*, December 1973, volume 22, p. 153.

This study presents the results of an investigation of research and development expenditures and output per manhour in twenty-one U.S. manufacturing industries from 1948 to 1963.[1] Two sets of data were needed for each industry included in this study. First, a consistent index of productivity was constructed covering the entire period.[2] From this we computed the average annual rate of change in output per manhour by fitting the compound interest curve by means of least squares over periods of various lengths. Second, we needed

[1] The Industries used were: Food and Kindered Products (SIG 20); Tobacco Manufacturers (SIG 21); Textile Mill Products (SIG 22); Apparel. Other Textile Products (SIG 23); Lumber and Wood Products (SIG 24); Furniture and Fixtures (SIG 25); Paper and Allied Products (SIG 26); Printing and Publishing (SIG 27); Chemicals and Allied Products (SIG 28); Petroleum and Coal Products (SIG 29); Rubber and Plastic Products, NEC (SIG 30); Leather and Leather Products (SIG 31); Stone, Clay, and Glass Products (SIG 32); Primary Metal Industries (SIG 33); Fabricated Metal Products (SIG 34); Machinery, Except Electrical (SIG 35); Electrical Equipment and Supplies (SIG 36); Motor Vehicles and Equipment (SIG 371); Aircraft and Parts (SIG 372); Instruments and Related Products (SIG 38); and Misc. Manufacturing Industries (SIG 39).

[2] In order to calculate rates of change for each industry we first had to establish their annual output. For this we used FRB and Census data. We then multiplied the number of production workers in each industry by the average weekly hours to give us our labor input by industry using BLS data. The ratio of the results of the above two steps provided the desired output per manhour series. We then divided by the 1958 value to give us an index of output per manhour (1958=100).

a measure of R & D expenditure intensity. The ratio of R & D expenditure to value added in each industry was used 'on the assumption that the larger the industry, the greater the research effort required to bring about a given improvement in productivity'.[3]

Both R & D expenditures intensity and the $\log_{10}$ of R & D expenditure intensity for 1953, 1958, and 1963 were correlated with the average annual rate of productivity change for seven five-year periods, three ten-year periods, and one twenty-year period. The results are presented in Table I.

Beginning in 1953, when reliable annual industry R & D expenditures first became available, and until 1963, R & D and output per manhour were significantly related. Whether one posits a semi-logarithmic or linear relationship, the association appears to be a strong one.[4] Thus the assertion that R & D expenditures led to productivity advance seems to be confirmed. However, the argument that R & D lags rather than leads productivity advance cannot be rejected on the basis of the correlations found. The last four sets of pairings in Table I are not markedly different from the first three sets in which a strict lead hypothesis is entertained.

In the last quinquennium, 1963–68, no statistically significant relationship could be found. The lack of an association in the final period is, in itself, an important finding.

TABLE 1.—COEFFICIENTS OF LINEAR CORRELATION BETWEEN R. & D. INTENSITY AND AVERAGE ANNUAL RATE OF PRODUCTIVITY CHANGE

R. & D. year	Productivity change for period	Coefficient of correlation: R. & D. intensity	Coefficient of correlation: R. & D. log 10 intensity
R. & D. leads productivity change:			
1953	1953–58	*0.4995	*0.5281
1958	1958–63	**.5817	**.5540
1963	1963–68	.269	.2021
R. & D. partially lags productivity change:			
1953	1948–58	**.7286	**.7950
1958	1953–63	**.6227	**.5680
1963	1958–68	*.4821	*.4880
1968	1948–68	**.6506	**.5505
R. & D. lags productivity change:			
1953	1948–53	**.5951	**.5568
1958	1953–58	*.5978	*.4839
1963	1958–63	**.5948	**.5764
1968	1963–68	.2658	.1756

*Significant at the 0.05 level.
**Significant at the 0.01 level.

The explanation of this finding requires further research; a number of speculative hypotheses can be suggested. For example, it is possible that excessive Department of Defense and NASA sponsored research do not generate productivity advance.[5] It has been suggested in

[3] N. E. Terleckyj, 'Sources of Productivity Advance. A Pilot Study of Manufacturing Industries 1899–1953' (unpublished doctoral dissertation, Columbia Univ., 1960), p. 20.

[4] Terleckyj implies that the $\log_{10}$ of R & D intensity is a better measure than the simple intensity figure. N. E. Terleckyj, 'Component on Brown and Conrad's "The influence of Research and Education in CFS Production Relations"', in M. Brown ed., *The theory and Empirical Analysis of Production*, NBER, *Studies in Income Wealth*, Volume 31 (New York, 1967), p. 376.

[5] R. A. Solo, 'Gearing Military R & D to Economic Growth', *Harvard Business Review*, Nov.-Dec. 1962, pp. 49–60; E. Mansfield, 'Contribution of R & D to Economic Growth in the United States,' *Science*, February 4th 1972, pp. 477–86; and William N. Leonard. Research and Development in Industrial Growth', *Journal of Political Economy*, March-April 1971, pp. 232–56.

the popular literature that R & D projects in the past decade have not provided a sufficient return in some industries.[6] The lack of a significant correlation between productivity change and R & D, and profits and R & D may indeed offer an explanation. Insufficient care may have been exercised in choosing R & D projects, and thus, both productivity and profits may have been low. Another possibility is that the R & D data which firms reported to the NSF during our last quinquennium included a greater proportion of expenditures that economists would not expect to generate productivity advances: (1) expenditures prompted by a desire to minimize tax liabilities; (2) expenditures for marketing purposes and/or product differentiation (the red wrapper effect); and (3) boondoggle R & D projects undertaken to attract government subsidies. A final hypothesis that economists may be loathe to embrace is the view of a stationary state. It is possible that the United States economy is experiencing a twentieth-century *climacteric*. The testing of these and other hypotheses is crucial, not only for practical governmental and industrial decision-making but also for a better understanding of the functioning of the economic system.

Muddling Through: Government and Technology

By William D. Carey

From *Science*, April, 1975, volume 188.

Not everyone is sure that technology generates greater social benefits than costs. What is quite clear, however, is that a sick national economy is not going to create needed jobs, nor improve productivity so that we can afford to help others, if its technological capacities are not up to it.

Government tends to imagine that a mystery called the market system defines the level and quality of technological enterprise. It is true that private decision-makers balance opportunities against corporate risks in estimating returns from innovation. But the environment of private decisions is conditioned heavily by government's attitudes and behavior. There is scant evidence that the federal government has the policy machinery to guide its actions as they affect the environment for innovation.

For a time it looked as if government had caught on to the need for explicit public policies toward technological vitality. That was in 1972, when Michael Boretsky of the Department of Commerce showed that the United States was fast losing its lead in high technology exports. A presidential message went to Congress on science and technology, and whatever defects it had were redeemed by flashes of comprehension as to the need to encourage innovation. To test incentives for risk-taking, the National Science Foundation and the National Bureau of Standards were assigned new responsibilities. Thereupon, Carey's law became operative: that the half-life of a federal experimental program is about two and a half budget cycles. The NSF's program has been practically shelved. The Experimental Incentives Program in the Bureau of Standards has launched promising partnership

[6] 'A Squeeze Hurts Lab Spending', *Business Week*, May 8th 1971, p. 94.

experiments with regulatory and procurement agencies, yet its future is uncertain. So it goes, while the economic indicators fall and factions quarrel over the mix of fiscal antibodies.

The energy predicament dramatized the fragility of a technology-dependent economy. A materials crisis would teach us an even more emphatic lesson. The success of our Free World partners in invading our domestic markets thanks to our export of technological and managerial know-how, has begun to make us thoughtful. But when we hunt for a public policy framework within which technological vitality can be regenerated, we cannot find it. This is one place where presidential staff work in science and technology can stand strengthening.

Government may imagine that it is neutral toward the rate and quality of technological risk-taking, but it is not. The regulatory system alone is pervasive and here to stay, but regulatory policies aimed at the public interest rarely consider impacts on innovation. Standards-setting activities, important as they are, need not force distortions on technological compliance. Changes in tax treatment of industrial research and development, if approached narrowly, can choke off outlays for innovation and trigger even more exportation of R&D and know-how.

Government is not against technological innovation. But the habit of muddling through leaves American technology at increasing risk. Government should have policy machinery to align its industrial growth policies with its regulatory, taxing, R&D, and procurement policies so that discontinuities are referred. With this goes a need for better governmental research on the dynamics and performance of the technological enterprise in the United States, aimed toward a baseline for good policy analysis.

We have found out that compulsive technological drive is not the right answer. But we need also to know whether unintended governmental constraints are inducing adverse choices in industrial risk analysis at the expense of innovation. Now that we are in deep economic trouble, the question is less academic than it might have seemed when the nation's economy had its seasons in the sun.

Briefing: Science, Technology, and Inflation

By C. Holdan

From *Science*, October 4, 1974, p. 35.

One of the 12 "presummit" meetings to prepare for President Ford's big anti-inflation talk-in scheduled for 27 and 28 September was devoted to the question of how science and technology can contribute to driving down inflation. At Ford's request his science adviser L. Guyford Stever, head of the National Science Foundation, assembled a group of luminaries, including former science adviser Edward E. David, for 2 days of brainstorming on 18 and 19 September at the Cosmos Club. The group addressed itself to the role of science and technology in three broad areas: (i) manufacturing, materials, and energy; (ii) agriculture and food; and (iii) health services. One of the predictable sentiments of the group was that a flourishing R&D establishment is definitely anti-inflationary because it leads to the

development of more efficient and cost-saving processes. There was also agreement that science and technology would be better at bringing long-term rather than short-term relief, and that the inflation battle, as former National Bureau of Standards director Lewis M. Branscomb said, is going to be long, slow and tough."

The manufacturing task force observed that wrestling the economy into shape would require more emphasis on the "mundane," that is, immediately useful, technology over high technology. Other suggestions had less to do with technology than with the manipulation of existing mechanisms such as tax and antitrust policy to stimulate innovation, and with an assessment of environmental and safety laws. These laws have forced companies to divert large amounts of money that might otherwise be used to increase productivity and efficiency, and, as one expert said, some of them lead to only marginal benefits. As an example, he said, the requirement that automobiles be fitted with expensive bumpers that can withstand 5 mile-per-hour impacts did not provide enough added safety to justify the expense in times like these. In such times, he said, we must ask ourselves nasty questions, such as, "What is the dollar and cents value of a marginal life saved?"

The health panel pointed out that a number of "perverse incentives" were operating to raise costs, such as reimbursement schemes that encourage the use of the costliest facilities, and pointless rivalry among solo practitioners that should be replaced by constructive competition between health delivery systems. Kerr White of Johns Hopkins University said that all forms of medical intervention need to be evaluated. Thorough annual physical examinations for everyone, he said, generated a lot of useless and costly data where as selective screening would be almost as effective and far less expensive.

The agriculture group, whose spokesman was Vernon W. Ruttan of the Agricultural Development Council, remarked that few savings can be made in farm production because the substitution of fossil-fuel power for human labor and the use of chemical fertilizers and pesticides have made farming just about as efficient as it is going to get. But there is still much food wastage and therefore room for increased efficiency in the processing and distribution links of the food chain. Here again, though, safety and anti-pollution regulations are eating up capital that could otherwise be used in improving efficiency and productivity in manufacturing.

Clearly, similar findings from the other presummit meetings will be forwarded to Ford. In the short term they spell increased unemployment as belts tighten; in the long term the question may be raised as to whether the nation will ever be able to afford living with a policy of continuous growth while at the same time making the investments necessary to ensure the maximum health and safety for all its citizens.—C.H.

Part B – R&D Decisions in Industry

"Industrial R&D – 1980"
(By the Long Range Planning Service Stanford Research Institute, Menlo Park, California, 1967)

EXECUTIVE SUMMARY

Total R&D expenditures, now $24 billion annually, are expected to grow more than 6% per year, reaching $53 billion in current dollars in 1980. By then, spending for industrially performed R&D, growing about 7% annually, could approach $40 billion, compared with $17 billion in 1967. In 1980 industry will likely fund slightly more than 50% of the research it performs, compared with little more than 45% in 1966.

• Annual R&D performance growth rates for many individual industries will decline over the next 13 years, compared with the 1955-1965 period. During 1955-1960 the average annual growth was 17%, but it has decreased considerably in recent years. Seven industrial groups perform approximately 90% of industrial R&D, with aircraft and missiles, electrical equipment and communications, chemicals, and motor vehicles and other transportation heading the list. In the future, professional instruments, drugs and medicine, office machinery, and tobacco will be the R&D growth leaders, all expanding their outlays by more than 10% yearly.

• The social and psychological sciences, emphasizing man's place in today's complex technological society, will receive increased attention. Federal funds for such activity will increase from an estimated $380 million in 1967 to around $1 billion by 1980. Since R&D in these disciplines does not require as large outlays for equipment as the physical sciences, the impact of social science research on the other fields will not be monetary. Instead, the results of these studies will affect the nature of R&D programs in the physical, engineering, and biological sciences.

• Approximately 800,000 scientists and engineers could be actively engaged in R&D by 1980, with about 70% of them in industry. These personnel will utilize computer terminals to perform many routine functions of data collection and processing. The time saved will release researchers for more creative thinking, enriching their overall productivity.

• Instrumentation, now accounting for 20% of R&D expenditures, could account for 25% to 30% by 1980. This would represent $13 billion to $16 billion. Instrumentation will become more complex, and will incorporate computer logic, either in the equipment or as an adjunct. Additional applications of computers will include scientific information handling; information switching centers will be tied to storage depots throughout the U.S.

"R&D"

By Howard Wolff

From *Electronics,* October 17, 1974, p. 178.

It all starts with research and development. And at R&D establishments—ranging from the big corporate and university centers all the way to the smaller, less formally structured installations—the money crunch is forcing the focus away from blue-sky projects to applications that will take the form of products—and profits.

As the physicists, chemists, metallurgists, and electrical engineers in the laboratories find themselves thinking more and more of filling spaces in product lines, their superiors are thinking more and more of organizing their R&D efforts along the lines of profit centers. In some places, such thinking already has resulted in a crisp, somewhat more businesslike R&D organization.

This thinking will be encountered at a cross section of laboratories across the U.S. But in California's Silicon Valley, south of San Francisco, three companies might serve as classic examples. Although they have more differences than similarities. Hewlett-Packard Co., Fairchild Semiconductor, and Intel Corp. have at least three strong R&D characteristics in common:

They all consistently plow back at least 10% of their annual sales revenues into R&D.

They all support fairly large R&D groups in relation to their size. H-P, with about 28,000 employees, has 1,900 in R&D; Intel, with 2,000 in its work force, has 200 of them in R&D: Fairchild's numbers are 800 out of 27,000.

They all write off R&D very conservatively, substracting outlays from the net income for the year they occur rather than when a resulting new product begins to make money.

At H-P, Dan Lansden, administrator of the corporate laboratories, says that R&D is highly product-oriented and aimed at the practical application of theoretical ideas rather than at extending the theory. To this end, H-P keeps 270 researchers at corporate H-P labs in Palo Alto, while the other 85% are scattered among the product-development groups that are a part of each of the company's 20 divisions.

The interface is effected this way: programs in the operating divisions are designed almost exclusively for development of specific new products or improvement of existing products. The corporate laboratories' primary mission, says Lansden, is to give technical support to those efforts, chiefly through exploration and application of new technologies.

At the divisional level, development work is performed by small project teams, consisting typically of electronic and mechanical engineers and an industrial designer. But throughout the course of a project, the team gets help and insight from manufacturing engineers and marketing personnel. An important factor is early involvment of these experts.

The division's engineering manager and general manager decide whether to take a product to advanced development or production.

They act after receiving a preliminary feasibility study made at the direction of the section manager for a particular line. The decision on a product's feasibility is based on an assessment of its total R&D cost, probable market, target price, sales per month, profit per year, the number of years it will make money, and the total net profit.

Back at Palo Alto, says Lansden, things are a bit less formal. "For one thing, we call what we do 'investigations' rather than 'projects.'" The objectives are a bit more free-form, but the investigation must ultimately appear to have some chance of getting to market. And where the time scale for a divisional research project is about three years from idea to product, the scale in the H-P Labs is two to five years from concept to prototype product feasibility.

As a rule, Palo Alto takes new technology investigations only to the point of feasibility; they then must be sold to a particular division that would carry on to the prototype and new product stages. "But the division general manager is under no obligation to buy the results of a particular investigation," Lansden explains. This places the H-P labs in the position of making sure that what they do is applicable by their "customers."

THE $10,000 CEILING

Decisions on how far to carry an investigation depend on scale. The departmental research manager alone can allocate up to $10,000 on any particular project. Anything over that must be approved by Bernard Oliver, director of corporate R&D. "This gives us a little more flexibility," says Lansden. "For one thing, it allows the investigator and the department manager to do a little preliminary study on a particular technology before recommending any big allocations. For another, it gives the departmental people a chance to prove a particular technology worth pursuing if there are doubts within the labs as to its practicality."

The same sort of split between central and divisional R&D is practiced at Fairchild, except that there is a little more centralized control. Some 50 to 75 researchers at corporate headquarters in Mountain View investigate technology that the corporate officers believe will result in either new products or the enhancement of old ones. Thomas Longo, who is vice president and general manager of the Digital Products division, says, "The ideas for which way to go are all over. What it comes down to in the long run is separating technical feasibility from economic feasibility."

That decision is made on the corporate level by Longo and James Early, director of corporate research. At the division level the decision is made by the general manager, with his research manager and Early and Longo as consultants. As for pilot lines, separate production facilities are maintained for the corporate research group. When production feasibility is proved, the project is either transferred into a division's production lines (as with Isoplanar) for further work by divisional researchers on yield improvement, or a separate division or group is set up around the pilot line (as happened with C–MOS).

At Intel in Santa Clara, the story is quite different. One reason is that the company specializes in semiconductor memories, unlike broad-based IC maker Fairchild or instrument maker H-P. Another is that most of Intel's corporate executives are scientists with doctorates in a

number of disciplines who came from companies with separate pilot and production.

So at Intel there are no pilot lines, says Andrew S. Grove, vice president for operations, and no development labs. His explanation: "For years I worked for a company that was a technological leader, but could not translate that leadership to the production line. When I came here [*Electronics*, Oct. 5, p. 86], I resolved that one place we were not going to screw up was in translating technology into production." For this, production workers must understand the needs of researchers, who in turn must understand the needs of production. The best way to achieve that mutual understanding, says Grove, is to have them share facilities.

"When visitors ask me where we do our production-feasibility studies," says Grove, "I show them our 'invisible' pilot lines scattered in bits and pieces all over the plant on equipment that isn't being used for production at a given moment." And those invisible pilot lines may shift around depending on where the necessary equipment is available.

"Over the short term it's inconvenient to both production and research people," admits Grove, "causing all sorts of glitches in their schedules. But over the long term it's meant terrific savings in dollars and in the amount of time it takes to move along the learning curve. So any temporary setbacks at the beginning are offset by the tremendous gains we make at the end."

In some companies, the research group also serves as a technical consulting service for the rest of the company's divisions. That's the case at Control Data Corp. in Minneapolis, even though the central research facility is primarily charged with looking into new areas of opportunity for the mainframe maker. John Baird, senior vice president for research and development, says that, as a result, "We've gotten the company into several other lines of business—semiconductors and plasma displays, for example."

The way R&D is organized at Control Data, explains Baird, "We take a technical idea and develop the technology through what we call applied research until it is in a position to be transferred to an operating group. We'll carry it as far as necessary to make sure the transfer doesn't fall through a crack. This, in some cases, involves production of prototypes, and in some cases the production of considerable product. But ultimately we want to transfer it out of research because we don't want to become an operating division."

Control Data has another unique method of keeping technology from falling through a crack. When a project moves from the lab to an operating division, the entire group working on it often is also transferred. In the past year, this occurred twice. The first time, 14 researchers went to the Aerospace division with their plasma display. "It went there because it's being sold to the military; we'll develop about $2 million in orders for that product this year," says Baird. The initial product was developed entirely in R&D and, because "that was an area where we had a lot of trouble getting anybody in the company interested in the product," the research division even sold displays outside Control Data.

The second transfer came when 26 employees were moved lock, stock, and image-comparison system into the Military division. While

most of the project is classified, the system automatically compares pairs of photo or radar images and prints out the difference between the two. "We've also done some work with this technique in chest X-rays," says Baird. "By comparing two taken a year apart, we can print out a photograph of the differences to show if any cancerous growth has occurred."

Control Data's operating divisions do relatively little research. But since they're charged with keeping product lines up to date and with developing new products, they do some. They follow the same general procedure as the central facility for funding: the division's chief engineer proposes projects for review by his general manager. Then it is bucked up the line to the group chief in peripherals, systems and services, or marketing.

Research expenses are written off each year. "The only R&D we defer," says Baird, "is that part of the development on a new product which corresponds to that part which is leased—and written off over the lease period. On a new computer system, for example, we might lease 40%, so 40% of the development cost is deferred and written off over the lease period. The same thing happens with marketing expenses."

The company's central research activity derives 30% to 40% of its support from corporate funding; the rest is split between outside R&D funded predominantly by the Government, and interdivisional work orders. Control Data spent about $164 million in 1973 for what it calls total technical effort—exploring advances in technology for new products and services, as well as applications of computer technology. Of that, R&D expenditures came altogether to $48.1 million. "So far as I know," says Baird, "I've never been stopped from funding anything I wanted to fund.

BUSINESS IS BUSINESS

It's easy to conclude that corporate R&D groups are focused sharply on saleable products—but what about institutional research organizations? The answer, says Sid Bass, director of electronic research at Chicago's IITRI, is that while it's usually run on a not-for-profit basis, institutional R&D is a competitive business. "We seek our business competitively, and we get paid for it," says Bass. "And as any business should, we start in the market, the R&D market. We determine what the needs are—and they may be services, not products, that have their origins in some policy or national goal," he says. "We look at where the needs lie, and who intend to spend the money." That's why the biggest customer at the Illinois Institute of Technology Research Institute's division is the Government, which supplies 80% of the contracts—65% of IITRI's business is from the Pentagon, 15% is from nondefense agencies. Industrial firms provide the rest.

Bass's division is finding more and more of the needs it seeks to fill in what he calls services: performance prediction of devices or systems, experimental and analytical evaluation of competing products, and comparing and reviewing specifications, to name a few. A full two thirds of the division's work is now service-oriented; the remainder consists of pursuing conceptual developments toward hardware itself.

"The electronics industry in this country serves its own industrial needs internally," says Bass. "There are very few secret processes that are proprietary and protectable, so the engineering that goes on in short-term, usually aimed at the next product cycle. And if I owned a company, I'd want to keep that kind of work inside." As a result, says Bass, most of the IITRI division's commercial research work is for non-electronics firms: "Electromechanical companies find themselves having to deal with a new technology on a transient or peripheral basis. Our work for them is generally on specific problems."

Things were not always thus for IITRI. It has invented, conceived, and developed hardware, such as its pioneer work in magnetic recording 30 years ago. "That development was second only to the transistor and atomic bomb in terms of impact, and it was invented here," says Bass. But recent attempts have seen IITRI outstripped by electronics firms. "The lifetime of a preeminent position, even if it arises, is too short," sighs Bass.

He gives this example of the inappropriateness of conducting basic research at an R&D institution: in the late 1960s, IITRI went all out in its work with surface-wave technology. "We invested heavily in facilities to make devices and succeeded only minimally in selling our research," recalls Bass. "For as soon as the technology payoff was apparent, the companies that felt they could get a product yield overwhelmed us. They invested more in a few months than we could afford to spend over three or four years."

"Research, even in the applied form, is increasingly hard to sell," he continues. "More and more of it is being done in the product-development labs of commercial firms. And there's so little market for basic research that we don't even bother to look for it."

That gloomy picture is not repeated in another university-sponsored R&D operation, the one at Massachusetts Institute of Technology in Cambridge, Mass. There, says Albert G. Hill, vice president of research, every faculty member is expected to do research in his field, either independently or as a part of a team. And it is the faculty member, most often, who decides what his research will be. About 90% of the funding is from the outside—mostly the Government, but also from individuals, foundations, and industry.

About $70 million worth of research is on the MIT campus, about $25 million of it with a strong electronics orientation (though Hill says it's difficult to determine where electronics research ends and where research that uses electronics begins). In addition, about $70 million worth of electronics-related research is going on at MIT's Lincoln Laboratory, which unlike the campus centers, is permitted by university rules to handle classified work for the Defense Department.

But on the whole, the atmosphere at MIT seems to be one of individual initiative—even extending to funding. Hill explains that "we aid faculty in getting research support since we know where the foundations and Government agencies are." However, over a period of time a professor will develop his own contacts among supporters, so most of the work in preparing research proposals is done by individual faculty members. MIT funds some research directly, but more often the individual researcher or group helps get its own funding. An Office of

Sponsored Programs represents the MIT administration in processing proposals, negotiating contracts, and making sure a contract is not overspent.

Recently, the university played its direct-support role with the new energy laboratory. In this case, the idea came from the administration, with Hill instrumental in formulating it, when the energy crisis struck. MIT felt that there should be a lab devoted to over-all energy policy, says Hill. And since it's difficult to determine just which Government agencies are responsible for energy action and which industries are hardest hit, MIT has yet to find a patron for the lab. It simply dipped into tis own general funds to set up the facility. Hill points out that perhaps $4 million worth of research on the MIT campus is related to energy, but that work is splintered among many contracts with no cohesion. It would take $5 million a year to fund a central energy lab that could pick its own projects with appropriate reviews.

It would seem that the only R&D rule at MIT is to do your own thing, but that is not quite the case. Besides the structure against classified and proprietary research—except at Lincoln Lab—there are other rules. The work must be state of the art or pushing it; the researcher and the university must have the capabilities required to do the work; and MIT, as a nonprofit institution, must not guarantee anything, such as performance parameters.

It's also expected that research will include student participation. Although that is not a strict rule, promotion, tenure, and salary increases depend in part on the number of students a professor has, so the pressure to include them is strong. Hill says that he has been forced several times to return proposals that have not been related to education.

Once a research project is underway, its results, interest, and value are "pretty much judged by the faculty member's peers," says Hill, rather than by the university. If a department has one of its members doing something routine, MIT wouldn't take a strong stand against the work if it were funded from outside. But if it burdens the university it would be discouraged.

While most research is done by individuals, Hill says, "Our people follow the news and we have a marketplace too, though it is less obvious than in industry. We are related more than we want to believe to the outside world." And there is interaction between researchers: a member of the nutrition department might have an idea for an instrument needed in his work and, with the help of a colleague in the EE department, develop it.

Once an MIT research project is finished, the results are generally published in a research journal—MIT does no development. If the research has led to a potential product, the researcher might obtain a patent or a business might be established around it. This work is handled by the MIT Development Foundation Inc.

A CRACK AT BELL

If there is still any doubt that R&D is going in new directions, consider what might be in the future for mighty Bell Laboratories. On file with the Federal Communications Commission are two internal reports on the operation of the labs. While lauding the work done

there—with its $500 million-a-year budget, Bell Labs averages two and a half patents per working day, says an AT&T staff study—both the AT&T staff study and one done by Bell System operating company officials are unhappy about supervision of the lab's work. The staff study recommends tighter direct control by AT&T.

Saying that R&D decisions are reached through "compromise and concessions," the report recommends giving AT&T officials authority to make R&D decisions, and also wants to let the marketing department have some role.

The times, as both studies point out, are changing. □

"Patterns of Impact and Responces in Research and Development in Industry: Summary of a Study"

By Guy Black

From George Washington University Program of Policy Studies, Washington, D.C., May, 1974 (Monograph 27).

The very scale of R & D and of Federal funding in industry raises the question of what impact changes in the level of Federal R & D funding might have on the pace of innovation, and on the contribution of technology to the national well-being. In particular there has been no conclusive evidence as to the impact on private funding. Previous studies had suggested that an increase in Federal funding might sometimes decrease private funding, but that it might sometimes produce the opposite result. Two early investigators, Blank and Stigler, read the data as indicating substitution of Federal for private R & D funds when government placed R & D contracts with industry. Black, and later Maertens, concluded that there did indeed seem to be substitutive effects in industries that were heavily oriented toward the government as a customer, such as aerospace and electronics. However, in low technology industries it appeared that the government might successfully undertake R & D pump-priming. The parameter of effect, called a multiplier, seemed to vary widely according to industry and the organizational mission of various industrial R & D groups.

It seems not possible to get very far with a question as specific as the Federal-private interaction without considering R & D in a border setting. While Federal funding may indeed have some impact on private funding, it is often a secondary consideration, and changes in private funding are primarily due to other causes. Until these are accounted for with reasonable success, it would not be possible to estimate residual effects attributable to changes in Federal funding with any accuracy. Only for a few firms and industries is the change in Federal funding of such overriding importance that it can be reasonably estimated without also taking account of other effects.

Knowledge respecting R & D leaves much to be desired. Much of the literature expounds or rationalize the "art" of R & D management, or of using R & D in a large context. As of 1959, Professor Richard Nelson could, from a literature survey, identify and state briefly most of what still passes as conventional knowledge on R & D. While a start

has been made at subjecting traditional ideas to the tests of econometrics, as well as estimating parameters, the number of points where our "knowledge" with respect to R & D is scientifically respectable is still quite meager.

One framework for econometric investigation can be an extension of the model of the firm with an R & D program. The economics of R & D rests largely on a number of fundamental propositions, that R & D transforms R & D inputs into outputs in the form of knowledge and designs, that its rewards follow in time when expense of R & D is borne so that it is inherently an investment, that the free-floating nature of knowledge with the attendant impossibility of restricting it to the creator have given rise to the internalization of most R & D in industry, and that the laws of supply and demand, as they can be applied to an internalized activity, are applicable to R & D. The model that is suggested by economics is one of economically rational R & D behavior: that is to say, we hypothesize that firms interpret their opportunities, constraints and business environment intelligently and increase, decrease, or reallocate funds for R & D so as best to advance the purposes of the firm.

These principles account for most of the special attributes of R & D, and differences among various business environments—for example, where there is a vast amount of unexploited technical knowledge and where there is not; where the customer is private industry, or big-system government; where firms do not compete through product superiority (e.g. public utilities), and where product characteristics are a principal mode of competition.

During 1971–73 a study was undertaken at The George Washington University with the objective of obtaining additional data on R & D in industry. It was to be based on information supplied by executives in a large number of firms and industries, in response to mail inquiries, or personal interviews. Much of the information supplied in interviews consisted of responses to a loosely structured set of questions. The responses to the question were scaled by the interviewer, and the set of scaled responses became the statistical base for the study of analyses.

Quite a bit of effort was spent attempting to collect somewhat elaborately structured data on R & D expenditures, and to obtain time series on R & D expenditures reaching back to 1960. While not as many data were obtained as was hoped for, many firms supplied the information desired, and made possible considerable econometric analysis. R & D data has also been obtained from mail responses and personal interviews and financial data from annual reports or S.E.C. records. Data were normalized for company size by using net plant and equipment as the scale variable.

Part of the resulting information was used to examine a pair of questions, how does the financial performance of business organizations affect the funding of R & D and second, how does the conduct of R & D affect financial performance. Financial performance has been represented by changes in the net worth of the company—number of stock shares outstanding times price per share.

With net income, sales, working capital, liquid assets, market value of stock options exercises, company R & D funding and government

R & D funding as an independent variables .34 of the variance in net worth could be explained in the statistical sense. However, adjusted for degrees of freedom a simpler model with only net income, sales and working capital as independent variables had an adjusted r-square of .35 and all variables were significant. Substituting government R & D for sales increased r-square to .38, but adding private R & D funding instead made no improvement. It appears that stock valuation is not changed by changing the level of private R & D funding.

When the amount of private R & D funding was the dependent variable, with net income, net worth, sales, working capital, liquid assets and government R & D as independent variables, only sales and working capital were significant, and r-square (adjusted for degrees of freedom) was .33. Government R & D funding was not significant. Most of the explanatory power was in the sales variable. A model with only sales and working capital as independent variables produces an adjusted r-square of .43, with both variables significant. The adjusted statistic was worsened by adding government R & D funding. Thus, it appears that the traditional R & D/sales ratio prevails, is modified primarily when a company's working capital position becomes poor, and no evidence of a direct effect of government R & D funding or private R & D funding were shown.

The same issues were examined econometrically with an enlarged body of data obtained for the period 1960 to 1971. A total of 256 observations were available. Models were constructed using financial variables, normalized for size and deflated, and R & D data for government and private funding. One set of models explored private funding as a function of government funding and trends for the entire group of firms. For all firms combined, government funding and the trend term explained 42 percent of the variance in private R & D funding. The addition of "IR & D" as an independent variable raised the variance explained to .70, undoubtedly because many of the firms were large defense and space contractors.

When the data were broken down according to industry, some differences appeared. The combination of government funding and trend explained only .32 of variance in private R & D funding in the chemical firms, .92 in the machinery firms, .73 in the electrical machinery firms, and .99 in the aerospace equipment firms. Trend terms were particularly significant in the chemical and machinery industries but marginally significant or not significant at all in electrical firms or aerospace.

The government private interaction seemed to have changed during the 1960–71 period. In 1960 only .09 of variance in private funding could be explained by government fundings but the percent increased to .41 by 1962, and, after dropping slightly in 1963, rose to a peak of .69 for 1965. After that it declined irregularly to .31 in 1971. The low association for the earlier years may be the result of imperfectly recalled data (as well as smaller sample sizes) but the decline in variance explained in the later years suggests a deterioration in the government-private interaction since 1965. The regression coefficient (with government funding the independent variable) followed the same pattern

as the correlation coefficient, rising from .16 in 1960 to .70 in 1965 and declining irregularly to .43 in 1971.

It seemed possible that other financial variables such as sales, earnings, liquid assets, current non-liquid assets, current total assets, net working capital, net worth and earnings per share might help understand the 1960–71 pattern. Sales, a trend term, and other financial variables were tried, one at a time. Of the financial variables, net worth and working capital had the best, but not spectacular significance levels. The variance explained by financial variables alone ranged up to .23 for a model in which trend and net worth were the independent variables.

When these models were applied to particular industries, the results were mixed. For the chemical firms, earnings was the only financial variable that approached significance. For the machinery firms, the best result was obtained with the trend term and net worth as independent variables. For the electrical machinery firms, net worth was the most significant independent variable. Earnings per share was most useful for the aerospace firms. Additional models run with lagged and first-difference variables showed a weak association of private R & D funding with previous-year government contract funding, but a strong association with previous year IR & D funding.

Another approach to analysis was undertaken with a different set of data, obtained in interviews with 54 executives during 1972. The responses to questions posed during interviews were recorded on a classic type of psychometric scale and subject to statistical analysis. Topics included business condition impact, performance, policy configurations, internal adjustments in response to external impacts, including a spectrum of types of R & D. The 54 firms represented a wide range of industries, geographical locations, firm sizes, and organizational characteristics.

With this data, an effort was made to approximate as closely as the data permitted the models of R & D-financial performance interaction previously reported by Horowitz, Hamberg and Grabowski. Study data had to be taken as surrogates for the variables of these authors and the rating-scale nature of our data obviated direct comparison of parameters with the earlier studies.

Taking R & D, as a whole or some component, as a dependent variable, general business conditions were significant in over half of the models, sales, cash position and profits were significant somewhat less commonly. The attitude of respondents to general economic conditions was statistically superior to any single financial variable, as a general rule. General business conditions and cash position together could explain about 42 perecnt of R & D funding. Even for a subset of firms reporting some degree of impact from government R & D funding, general economic conditions was the most significant variable and sales and cash position were the most significant of the financial variables.

For the group as a whole, the average scores indicated that while the impact of adverse 1970–71 business conditions was felt most strongly as reduced volume of sales, other changes were often noticeable. Impacts, ranked from strongest to weakest, were: new technology; government policies and regulations; intensified competition; material and supply prices; wages and salaries. Over half of firms (58%) experienced a change in the competitive position in their industry.

Typically, adverse business experience resulted in compensatory moves on the part of management. The areas of response (from greatest to lowest) were: general belt tightening, capital expenditures; overhead activities; R & D activities; reorganization; deferred maintenance; product prices. In marketing and advertising, the response was minimal. About half of the firms experienced no impact from reduced government R & D funding or procurement, but among the rest, the effect was often quite strong.

Differing patterns of association can be observed when the respondents are divided as high or low respondents to selected questions. Thus comparing firms hardest hit by business conditions in 1971 with those less affected, it appears that the hard hit firms undertook a wider range of adjustments and belt tightening, including R & D; and especially cut back on product improvement R & D. The R & D cutback was however quite general with some shift toward efforts with a short-term payoff.

There does not appear to be a tendency to respond differently with respect to product-oriented or process-oriented R & D, or even basic research; rather, R & D programs seem to win or lose favor as a whole, preferences being reserved for efforts with the more immediate payoffs. In a factor analysis, all R & D-related questions grouped together in the principal factor. Another factor grouped long-run payoff efforts with positive loading and new product R & D with a negative loading. Another factor grouped image enhancement and stimulating customer interest by holding but new technical possibilities—two R & D activities considered to be particularly important for government-oriented firms.

In a second factor analysis the principal factor grouped image enhancement as an R & D purpose with impact of government procurement, and stimulating customer interest with government R & D funding. All the R & D impact questions were again grouped in one factor, indicating again the "unity" of R & D.

In the formulation of this study, it was hypothesized that R & D was one of many means by which firms might satisfy a variety of organizational needs. It was felt that firms might relate elements of their R & D programs to different functional activities of the firm, and, with changing business conditions, the link between an R & D program element and the germane functional activity would result in differential impacts on the various R & D program element, and a shift in program balances.

Firms did indeed report changes in R & D program balance as a result of business adversity, but this change seems to have been more in the nature of an across-the-board shift toward short-term payoff activities without basically disturbing the product-process program balance. It is clear from the comments of many executives that there is often a fairly rigid separation between product-oriented R & D and process-oriented work. Indeed, a fair number of R & D executives even refuse to call as R & D innovative change in process technology, though agreeing that quite a bit of it may be done by engineers attached to the production department. They consider as R & D what *they* are responsible for, and organizational barriers often exclude R & D groups from production technology other than that required by

new products or materials. One consequence is that firms sometimes lack enough organizational flexibility to shift R & D personnel along the product process continuum.

There is ample comment to the effect that in the past many firms supported R&D that was uncertainly associated with the firm's needs and objectives. Some firms may still not in fact make a very explicit connection between their product needs, process needs, and elements of the R & D program. Where the connection is weak, there is little reason for thinking that a change in needs and objectives will have much impact on R & D. The funding of R & D in industry has been impacted or constrained by financial pressure—suggesting the widespread survival of how-much-can-we-afford or the normal ratio of sales as funding rules.

Net worth as measured by stock values reflects investor assessment of the potential in the firm's future. The reason that R & D has been found to have so little impact on this assessment may be because of the secrecy surrounding levels—and changes in the levels—of R & D funding in most companies. Probably the impact of R & D comes from results—not the fact of R & D funding. Companies do, of course, glamorize their R & D in a general sense but apparently the glamour had no general impact on investor attitudes as of 1971–72. R & D is an investment in the future. The immediate effect of R & D is to worsen reported profits, and the financial community does react to profits.

With respect to government funding, the amounts are rarely secret, and the receipt of government R & D contracts is often taken as a portent of future production business, and there is no adverse effect on immediate profits. Thus, it is reasonable that the receipt of government R & D contracts should have a favorable impact on the investing community where private funding did not.

It does not appear that there is any direct, automatic relationship between the current amount of government R & D funding and the amount of company R & D being funded currently. It is even possible a company might receive an increase in contract R & D and at the same time reduce its company funding, not because it was using the government money as a substitute but because the government's future plans did not indicate any good opportunities via R & D. Such is the implication of comments from executives, to the effect that reductions in company funded R & D in government-oriented firms occur because of the reduction of opportunities to use R & D effectively as seed money for upcoming government contracts—principally because there are many fewer of such opportunities on the horizon. The aerospace industry tends to believe that there has been a fundamental, permanent reduction in the government market—that while there will always be armaments programs their firms must adjust to a fundamentally different scale of operations. Since their interest in R & D was strongly related to potential government contracts they have reduced their R & D. Financial stringency has sometimes forced even greater reductions than they might otherwise implement.

Generally speaking, this study did not find a strong basis for government-private R & D multipliers, and it appears that different, perhaps more complex models are needed that can be encompassed with available data if R & D is to be thoroughly understood. While the

quantity of data obtained have fallen short of the original target, it seems reasonably clear that even the target amount of data would have produced much the same result.

The tendency among firms is to manage R & D as a whole, to put considerable emphasis on the stability of R & D organizations, and implement that emphasis by allowing R & D management a large voice in program design. Are firms overlooking a useful approach to R & D planning by not systematically distinguishing needs for technological change in products and processes, and other needs that might be satisfied by R & D and structuring their R & D programs around the resulting profile of technological needs? It would appear that few firms think through their R & D programs in this framework, although the approach would appear to have much to recommend it.

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Part C—Impact of R & D Related to Size of Firms

"Fear Extinction of Inventor-Led Firms"

By Loebe Julie

From *Electronic Engineering Times*, July 28, 1975.

A problem of grave national importance—the 50 per cent attrition of high-technology, inventor-led electronic instrument companies—grows daily. Signs point to a 90 per cent attrition rate within a very short time unless immediate priority is given to the situation.

A gap between the American inventor and society and an even larger gap between the inventor and the government has always existed. A famous example is the case of Robert Goddard, father of modern rocketry and space travel. When the American government interrogated German missile experts about their secret World War II rocket developments, their answer was "Ask your Dr. Goddard. We got them all from him." Despite his ground breaking work, this American inventor was unable to get support from DOD. He succeeded only because the Guggenheim family and the Smithsonian Institute provided assistance.

For 30 years American inventor-led electronic instrument companies (Tier 2) have given this country a commanding lead over the rest of the world in electronic technology, defense and industry. Endless innovations and inventions—the modern oscilloscope, counter, digital voltmeter, operational amplifier, for example—have come out of inventor-led, small business Tier 2 companies.

For 30 years there has been enough commercial business to keep these companies healthy and strong in spite of the overwhelming marketing advantages of the large Tier 1 companies. Although in theory DOD was the biggest and best potential consumer of Tier 2 products, most of these companies never sold to the Pentagon and essentially got along without DOD business.

With the decline of the commercial market, Tier 2 companies *must* be able to sell to the DOD in order to survive. However, no matter how much they try, each calendar day brings the news that still another Tier 2 company has been turned down on still another DOD contract award.

Something can and must be done quickly if it is to be effective. A relatively minor change in attitudes of DOD engineers toward Tier 2 attempts to get contracts would save not only the Tier 2 companies but would benefit DOD. Current defense contracts award Tier 1 firms fifty-million dollars which only increase their present sales volume of one-billion dollars five per cent. The same fifty-million dollars awarded to Tier 2 companies would save 50 of them from extinction and keep them contributing to our technology.

As you read this, at least one contract award is being denied a Tier 2 company because of the negative attitude some government specifying engineer has about Tier 2. Tomorrow, another vitally needed award will be denied another Tier 2 company.

SUPPLIER HIERARCHY

Tier 1

A. Large business, commercial product, high marketing activity companies

B. Large business, mixed product, medium marketing activity companies

Tier 2

Small business, high technology, state of the art, design/manufacturing companies

Tier 3

Small business, standard product, "Me Too" companies

Tier 4

Small business, design-only (no hardware expertise), system companies

Tier 5

Large business, contractor, high proposal activity companies

Tier 6

Small business, manufacturing only "chinese copy" companies

Tier 7

Small business, inexperienced "bicycle shop" companies

"Research and Development Costs as a Barrier to Entry"

By Dennis C. Mueller and John E. Tilton

From the *Canadian Journal of Economics*, November, 1969, volume 2, p. 570.

The view that large firms are responsible for a relatively greater amount of inventive activity, and introduce proportionately more innovations than small firms has, for some time now, appeared under various guises in the economic literature. The classic statement of the hypothesis, that of Joseph Schumpeter, argues that relative (rather than absolute) size or market power is the key stimulus to entrepreneurial innovative activity.[1] Recent variants of the Schumpeterian hypothesis have stressed size alone,[2] diversification,[3] or size combined with oligopolistic rivalry [4] as the key determinants of innovations.

While space considerations do not permit a complete review of this literature, one important distinction between Schumpeter's hypothesis

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[1] Joseph A. Schumpeter, *Capitalism, Socialism, and Democracy*, 3rd edition (New York, 1950), chaps. 7–8.

[2] D. E. Lilenthal, *Big Business: A New Era* (New York, 1952), chap. 6; John K. Galbraith, *American Capitalism* (Boston, 1952), chap. 7.

[3] Richard R. Nelson, "The Simple Economics of Basic Scientific Research," *Journal of Political Economy*, 67 (June 1959), 297–306.

[4] Henry H. Villard, "Competition, Oligopoly, and Research," *ibid.*, 66 (Dec. 1958), 491–7.

and those of the neo-Schumpeterians must be noted. Schumpeter argued that the entrepreneur whose firm possessed some market power would lead in undertaking innovations, because he would be sufficiently free from the day-to-day struggle for survival which competition imposes to devote his energy to introducing innovations.[5] This freedom to innovate, combined with the financial capacity to innovate which stems from the flow of monopoly profits to the entrepreneur, makes him the prime instrument of development. In contrast, the neo-Schumpeterian hypotheses all seem to rely on some form of economies-of-scale argument. Only the large company, they argue, can undertake the great fixed costs of a modern research laboratory, achieve the degree of specialization necessary for adequate team work, benefit from risk-pooling by undertaking a number of R&D projects simultaneously, and sponsor basic research with reasonable expectations that the results will have commercial applications in areas of interest to the firm.

This paper contends that the validity of these hypotheses varies depending on the age or state of development of an industry. A new industry is created by a *major* process or product innovation, and develops technologically as less radical, follow-on innovations are introduced. In the following discussion, an industry is regarded as passing through four separate stages of technological growth: innovation, imitation, technological competition, and standardization. Any breakdown of this nature is, of course, somewhat arbitrary and unrealistic. Technological development, like other growth processes, is continuous. Yet, we feel that this breakdown has sufficient pedagogic value to warrant its introduction in this paper, which proposes to examine at each of these four stages of technological development the magnitude of entry barriers and economies of scale attributable to R&D.

I. THE INNOVATION STAGE

A new product or production process is invented, developed, and introduced into the market during the innovation stage. At this stage its technical and market potential is most uncertain. For until the invention is converted into an economically viable product or process, the innovator can have only very crude estimates of the returns he will reap from its introduction.

It is at this stage that Schumpeter's hypothesis should be most applicable. The "process of creative destruction" consists of introducing products and processes so radically new that even the monopolist cannot rest easy for fear of being wiped out by some innovating entrepreneur outside his industry.

Schumpeter's hypothesis has never truly been tested for this stage of the technological development process. Indeed, by focusing upon the most important technological innovations, it eleminates the possibility of testing by means of traditional statistical techniques, since, to ensure that they are sufficiently important to classify as Schumpeterian innovations, the researcher is forced to be selective, and hence nonrandom, in his choice of innovations. The researcher is thus limited to analysing a series of case studies which are always subject to question

[5] Schumpeter, *Capitalism, Socialism, and Democracy*, chap. 8.

and refutation by counter example. Still, the existence of a reasonably large number of examples which do not fit the hypothesis must be considered as fairly strong evidence against its general applicability.

In discussing the Schumpeterian and neo-Schumpeterian hypotheses, we shall make frequent references to case study evidence to support our arguments. It will be helpful in examining the plausibility of these hypotheses at this first stage if we further break the process down into the invention and development phases.

INVENTION

The bulk of the empirical evidence on the origin of major inventions suggests that the R&D laboratories of large corporations have not been an important source of major inventions. This is the conclusion Daniel Hamberg reaches after a rather extensive search of the case study literature.[6] Hamberg presents a number of plausible explanations for this finding: (i) large corporations generally prefer R&D projects promising short payoff periods rather than the lengthy projects usually necessary for major innovations; (ii) large firms frequently have a vested interest in the present technology and may hesitate to pursue major innovations which would displace currently profitable products; and (iii) highly creative scientists and inventors often find the teamwork atmosphere of many large corporate laboratories unattractive. This list can be further buttressed by the addition of what we shall call the communication and incentive problem in the large corporation.

The technical and scientific competence to judge the feasibility of a specific invention or perceive the inventive potential inherent in a given scientific advance normally lies with those working in a company's laboratory. A decision to fund an R&D proposal which, if successful, promises to produce a major invention must be made by someone fairly high up in the corporate structure. This individual is often too far removed from the laboratory to judge the technical merits of the proposal and, given the great risks involved and inadequate financial incentives, is likely to reject it. The problem of communications and incentives is much simpler in the small firm. Managers are only once removed from the R&D laboratory and reap a large portion of the benefits that any major innovation produces. In the large corporations, managers once removed from the R&D laboratories seldom have the authority or the personal incentives to initiate a development program on an important and radically new invention.

DEVELOPMENT

The same communication gap which may keep large firms from financing R&D activity directed toward major inventions may inhibit them from funding the development needed to convert a major invention into a commercially successful product or process innovation.

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[6] Daniel Hamberg, "Invention in the Industrial Research Laboratory," *Journal of Political Economy* 71 (April 1963), 95–115.

Illuminating insights into the problems large firms encountered in producing and developing major inventions are also found in P. E. Haggerty. "Innovation and Private Enterprise System in the United States," paper presented before the National Academy of Engineering on April 24. 1968. to be published in National Academy of Engineering, *The Process of Technological Innovation* (Washington, forthcoming).

This is particularly likely when the invention is made by small firms or individuals outside the company. The not-invented-here bias which at times causes corporate R&D directors to look askance at ideas not originating in their own laboratories may impede the adoption of outside inventions by large corporations.[7] This prejudice combined with the heavy financial risks and lengthy payoff period normally associated with developing and marketing any radically new invention often keeps big companies from acquiring the major inventions made available to them.

We have already noted the case of xerography, an innovation which a very small photopaper company developed after numerous industry giants had turned the inventor down. In investigating the forty-two innovations attributed to small firms and individuals in the Jewkes, Sawyers, and Stillerman study,[8] we found six were developed with substantial government assistance because of their military and strategic importance. Small firms (with annual sales of under $50 million) developed over half of the remaining inventions at least to the point where their commercial potential was clearly visible.

Relative size does not appear essential either. Enos' study of major petroleum innovations notes that the smaller petroleum firms developed those innovations based on inventions made outside the industry after the giants had turned them down.[9] The Dirlam-Adams study of the diffusion of the oxygen-injection process for making steel documents the leadership position taken by the smaller firms in adopting this innovation in the United States.[10] And so it goes. Despite some well-known exceptions, like nylon, the magnitude of R&D expenditures necessary to develop most innovations is not beyond the financial capacity of small and medium sized firms. Neither large absolute size nor market power appears to be a necessary condition for successful development of most major innovations.

This is a remarkable conclusion, when one considers that the Schumpeterian hypothesis should have its greatest applicability at the development phase of the innovation process. The heavy capital investments required to develop and market major innovations would seem

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[7] This bias against outside inventions often is so pervasive that even companies which adopt policies of actively searching for inventions from non-company sources apparently cannot overcome it. The Xerox Corporation has yet to come up with an important outside patent after over three years of searching, and other companies have had similar experiences with outside patent departments. Yet, Xerox is particularly sensitive to the potential of such patents since it obtained the basic patents on xerography from Chester Carlson (after many large firms turned him down). Xerox was a very small chemical firm (named Haloid) at the time it acquired these patents. The decision was made by the president of the company and the vice president for R&D. The latter had happened upon an article describing the invention while searching the literature for something new for the company.

[8] John Jewkes *et al., The Sources of Invention* (New York, 1959).

[9] John L. Enos, *Petroleum Progress and Profits* (Cambridge, 1962). On the other hand, Edwin Mansfield found that the percentage of innovations introduced by the four largest petroleum firms exceeded their share of industry capacity. See Edwin Mansfield, *The Economics of Technological Change* (New York, 1968), 109.

[10] This study has stirred up a bit of a controversy—chiefly regarding the performance of the US steel industry vis-à-vis the European and Japanese industries. The author's conclusion that the laggard performance of the industry giants in contrast with their smaller rivals tends to refute the Schumpeterian hypothesis seems to us on a solid ground, however. Walter Adams and Joel D. Birlam, "Steel Imports and Vertical Oligopoly Power," *American Economic Review*, 54 Sept. 1964), 626–55; Rebuen E. Slesinger, "Steel Imports and Vertical Oligopoly Power: Comment," *ibid.*, 56 (March 1966), 152–5; Walter Adams and Joel B. Dirlam, "Steel Imports and Vertical Oligopoly Power: Reply," *ibid.*, 160–8. Walter Adams and Joel B. Dirlam, "Big Steel, Invention, and Innovation," *Quarterly Journal of Economics*, 80 (May 1966), 167–89; Alan K. McAdams, "Big Steel, Invention, and Innovation Reconsidered," *ibid.*, 81 (Aug. 1967), 457–74; Walter Adams and Joel B. Dirlam, "Big Steel, Invention, and Innovation: Reply," *ibid.*, 475–82. G. S. Maddala and P. T. Knight, "International Diffusion of Technical Change—A Case Study of the Oxygen Steel Making Process," *Economic Journal*, 77 (Sept. 1967), 531–58.

to make the large corporation with its large capital flows the natural home for developing major inventions. However, it is the large capital requirements combined with the still substantial technical and marketing uncertainties that frequently keep these companies from underwriting the development programs. Here again we have the communication and incentive problem in the large corporation. The decision to go ahead with a major development project is at the very top of the company because of the magnitude of the outlays involved. The chief proponents of the development effort, usually the inventor and some of his close associates, are people further down the corporate hierarchy who may not have the incentive or the ability to instill in top management the vision and enthusiasm that they have over the potential of the invention.[11]

II. THE IMITATION STAGE

Once a major innovation is successfully introduced, much of the uncertainty disappears. Other firms can judge the innovating firm's success in overcoming the technical obstacles and also gauge the likely commercial potential of the innovation on the basis of the initial market reaction.[12] Occasionally the innovating firm has an airtight patent position and prevents other firms from entering the industry or adopting the process.[13] Generally, however, this is not the case. Even firms which can establish an exclusive position often choose to license new entrants and avoid a conspicuous monopoly.[14] Hence, one usually expects a rush of firms entering a newly formed industry or adopting a new process innovation shortly after it is introduced.

Follower firms first set up R&D programs to obtain a familiarity with the technology and science underlying the new innovation. They then attempt to develop their own variants of the innovation. Although we have called this stage in the development cycle the imitation stage, in fact, the firms which enter after the innovator do not attempt to reproduce the innovator's achievement exactly. Instead, they try to develop a version of the product or process which is as different or superior to the innovator's as possible.[15]

During the early period of entry and experimentation immediately following a major innovation, the science and technology upon which it depends is often still only crudely understood. The rudimentary state of knowledge about the relevant scientific principles can easily be comprehended by someone familiar with the area. R&D work undertaken to increase the stock of scientific knowledge may by necessity follow a trial-and-error or empirical approach. Typically, re-

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[11] The outside inventor has an even more difficult task trying to get a company's management to listen to him and carefully consider his proposals. This problem of communication is the chief obstacle the outside inventor faces in trying to sell his invention to corporations.

[12] The problem of estimating the market potential is obviously much more acute in the case of new products than of new processes.

[13] Polaroid has had such a patent position.

[14] The chief impetus for generous licensing policies in the United States is the antitrust statutes. Large firms in particular hesitate not to license other firms for fear that a judge or jury will later construe such behavior as a predisposition to monopolize.

[15] In the drug industry, for example, most innovations are followed by a large number of imitations which differ from the original innovations in a number of important respects. Imitators are motivated to differentiate their products to attract customers and to avoid infringing on the innovator's patents. See William S. Comanor, "Research and Technical Change in the Pharmaceutical Industry," *Review of Economics and Statistics,* 47 (May 1965), 182–90.

search is less amendable to the team work and highly specialized approaches of the modern large industrial research laboratories. Because of the newness of the area, the R&D worker often must devise his own experimental apparatus which tends to be rather crude and makeshift in nature.

All of this suggests that the large firm may not have an advantage over the small firm in undertaking R&D at this stage in the technological growth of an industry. The small firm can hire a few capable scientists and engineers and equip them to work as efficiently as their counterparts in the larger firms. The cost of conducting technically efficient R&D at this stage may be low enough to allow all but the very smallest firms to enter. The one major advantage the large firm conceivably might possess comes from its ability to pool risks. A large firm can finance a number of parallel R&D projects to improve on a major innovation, while a small firm can sponsor only one or two. The large firm can thereby reduce the risk that it will come up with no significant improvements. This advantage, however, must be weighed against the numerous disadvantages of the large corporate laboratory listed above.

The manager of the small firm entering a recently opened industry in response to a major technological innovation, knows that if his firm can successfully establish a place for itself, the rewards will be substantial. These small firms are usually technically oriented. Their managers are likely to have scientific or engineering backgrounds and may even participate in some R&D work in the laboratory. They certainly keep in close touch with developments in the laboratory, participate heavily in major decisions connected with the R&D program, and are rewarded in direct proportion to the success of the company. This situation must be contrasted with the decision-making process in the large firm where, as noted above, the responsibility for moving into new areas is separated from the technical competence to judge the potential in these areas.

While a fair amount of case study material has been gathered regarding the performance of large and small firms at the innovation stage, very little has been done on the history of an innovation immediately following its introduction. Research we are currently conducting on the semiconductor and photocopying industries does provide some evidence. Bell Laboratories, which developed and patented the original semiconductor technology necessary for transistor production, followed a very liberal licensing policy and made the technology available for a minimal fee to all interested firms. Both large firms, like RCA, General Electric, and Westinghouse, and firms that were initially small, like Texas Instruments, Transitron, and Fairchild, entered the industry. The latter contributed substantially to the technology of the industry [16] and in the process grew rapidly. Their ability to compete successfully with the large firms is demonstrated by the magnitude of their industry sales.

Despite the strong patent position of the Xerox Corporation, more than 40 firms have entered the photocopying industry since the de-

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[16] Lists of important semiconductor innovations and the firms responsible are found in: US Department of Commerce, Office of Technical Services, *Patterns and Problems of Technical Innovation in American Industry* Report to NSF by Arthur D. Little, Inc. (Sept. 1963), 150; and C. Freeman, "Research and Development in Electronic Capital Goods," *National Institute Economic Review*, 34 (Nov. 1965), 64.

velopment of xerography. Most of these new entrants like Xerox itself were originally very small firms, although a number of them were later absorbed by much larger companies.

This limited empirical support and *a priori* reasoning above leads us tentatively to conclude that technological barriers to entry and economies of scale in R&D are still comparatively small at this stage of an industry's technical growth. Small firms can be expected to enter and in a fair number of cases do reasonably well in advancing the industry's technology.

III. THE TECHNOLOGICAL COMPETITION STAGE

As the number of firms entering the industry increases and more and more R&D is undertaken on the innovation, the scientific and technological frontiers of the new technology expand rapidly. Research becomes increasingly specialized and sophisticated, and the technology is broken down into its component parts with individual investigations focusing on improvements in small elements of the technology.

Many of these developments work to the advantage of the large research laboratory. The popular conception of the modern industrial research laboratory as a well-equipped technology factory where R&D problems are subdivided into small projects and farmed out to teams of specialized scientists is a fair representation of R&D *at this stage of an industry's technical maturity.* Once a good understanding of the basic science underlying the process is achieved, this subdivision and specialization becomes feasible and often technically advantageous.

Since most firms in the industry are by this stage carrying on extensive R&D programs, small firms outside the industry find it difficult to enter and establish small R&D efforts which compete efficiently with the established firms. Added to the economy-of-scale problem that the small firm now faces is the heavy initial investment in R&D typically necessary to bring the small firm up to date technologically in the industry. The amount of unpublished knowledge—production techniques, characteristics of the materials used, reliability of various components of the innovation—the existing firms in the industry have acquired, often through production experience,[17] may be substantial. The ante for entering is further raised if the early entrants established strong, overlapping patent positions. This forecloses many otherwise potential avenues open to the firm [18] and forces it to invent around existing patents, adding further delays and expenses to the necessary initial R&D efforts.[19] Hence, we conclude that the barrier to entry in the form of required initial R&D outlays at this stage in an industry's technical development generally is quite high and discourages small firms from entering. Where economies of scale in on-going R&D activity exist, this conclusion is reinforced.

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[17] For a survey of the learning-by-doing literature, see Richard R. Nelson, Merton J. Peck, and Edward D. Kalachek, *Technology, Economic Growth, and Public Policy* (Washington, 1967), 89–109.

[18] If the firm chooses to obtain licenses from some of the other firms in the industry, then its entry expenses are effectively raised by the capitalized value of its royalty payments.

[19] A number of synthetic fabrics have been developed to get around basic patents held by pioneers in this industry. The extent to which these patents can act as a barrier to entry is illustrated by the experience of the rayon industry where a flood of new entrants followed the expiration of American Viscose's basic patents. See Jesse W. Markham, "An Alternative Approach to the Concept of Workable Competition," *American Economic Review*, 40 (June 1950), 349–61.

Small firms already in the industry may continue to do fairly well even with economies of scale in R&D if they have carved a niche for themselves (often producing a specialty product) and have protected it with some patents. A small firm considering entering the industry, however, must invest considerably more in R&D than the small firm already in the industry, since it has both to catch up in knowhow and establish a protective patent position to guard its own chosen interstice.

Frequently, especially in the case of product innovations, demand for the product (at prices that allow producers to prosper) greatly exceeds supply during the early growth period, and the many new entrants into the industry during the imitation stage do fairly well. Later, as production catches up with demand, however, competition eliminates the weaker newcomers. In particular, those firms falter which do not succeed in making significant improvements on the innovator's product or production process and cannot, therefore, attract customers either on the basis of quality or price. Their growth slows and eventually declines. Finally, they are forced to drop out of the industry. This whole process is accelerated by economies of scale in R&D, for then the stagnant firm tends to fall further and further behind its growing competitors technologically.

IV. THE STANDARDIZATION STAGE

Eventually, technological progress in the industry slows down and the production techniques become quite standardized. The necessary scientific knowledge for production is of the textbook variety. Many important patents of innovation and imitation stages expire and in general barriers to entry based on initial R&D requirements fall. Barriers that remain depend on the capital requirements necessary to establish efficient production and marketing organizations. Competition has shifted from technological to price competition.

V. ENTRY OVER THE DEVELOPMENT CYCLE

The discussion of this paper is summarized in Figure 1. The solid line depicts the weight of the entry barrier represented by the expected R&D costs for successful entry. The chief component of these barriers generally is the extent of economies of scale in the R&D process. These economies are relatively low in the innovation stages, increase gradually during the imitation stage, reach a peak in the technological competition stage, and then taper off as the industry's technology becomes standardized.

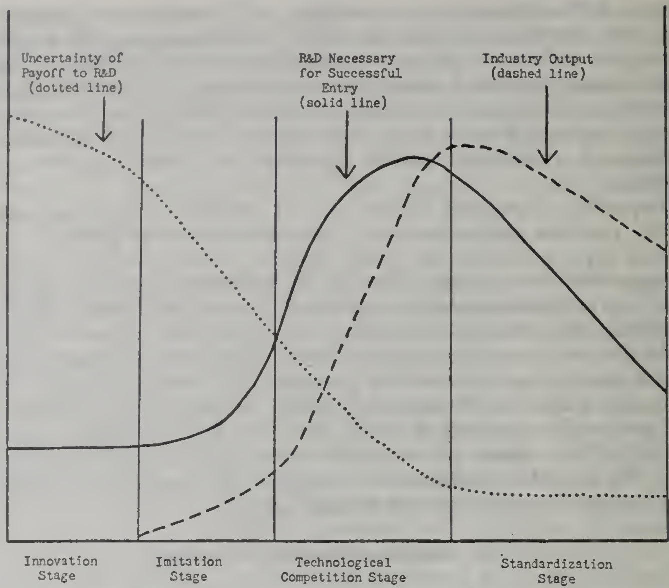

FIGURE 1.—Entry and the technological development cycle.

The second major factor contributing to R&D entry barriers is the accumulation of patents and knowhow on the part of incumbent firms. Even if there were no economies of scale in R&D, a late entrant would have to undertake more R&D than an average firm of its size in order to acquire information about the technology and to invent around existing patents.

The dotted line in Figure 1 reflects the uncertainty of payoff to R&D expenditures. While the absolute height of this curve is arbitrary, it does illustrate how uncertainty varies over the four stages of the development cycle. The greatest uncertainty exists at the beginning of the innovation stage when the pioneering firm or individual has very limited information about the innovation's potential. Uncertainty decreases as knowledge concerning technological and commercial aspects of the innovation accumulates.

A firm's decision to enter an industry is a function of all barriers to entry and the expected profits following entry. (The reader is reminded that we consider only R&D costs and uncertainty entry barriers here.) Future profits should be a function of industry output, which is depicted by the dashed curve in the figure. Again, the absolute height of this curve is arbitrary, the purpose of the curve is merely to show how output varies over the development cycle. In actual practice, its peak is substantially higher than that for the curve reflecting R&D entry costs.

In the innovation stage, the major deterrent to entry is uncertainty. The chief inducement is the potentially very profitable position a firm can establish as an industry leader should the innovation be successful

and the industry move up along the dashed curve. Indeed, a successful innovator's profits might well be regarded as his reward for having had the insight, daring or luck to have crossed the uncertainty barrier. Entry is generally most rapid during the imitation stage. Here the potential profits from establishing an early proprietary position are still fairly high, the uncertainty barrier is rapidly declining as outsiders are able to evaluate the technological and economic potential of a marketed innovation, and the R&D costs of entry are still rather low. During the last two stages, which normally are longer than the first two, entry is much slower. The R&D costs for successful entry act as a major barrier in the technological competition stage. In the standardization stage, declining sales and falling expected profits deter firms from entering.

"Research and Development and Other Determinants of Investment: Industrial Research and Development: Characteristics, Costs and Diffusion of Results"

By Edwin Mansfield

From the *American Economic Review,* May 1969.

I. INTRODUCTION

In view of the size of the current R and D effort in the United States [1] and its effect on other economic variables like the rate of technological change, the rate of investment, and the rate of economic growth, it is important that we obtain a better understanding of the nature and characteristics of the R and D programs carried out by industry, the factors that seem to be associated with the cost of individual R and D projects, and process by which the results of R & D become adopted and accepted. This paper summarizes very briefly some results of a number of related empirical studies of these topics.

II. CHARACTERISTICS OF R AND D PROGRAMS

The first study, by Michael Hamburger and I,[2] is concerned with the characteristics of industrial research and development. Despite the attention that this area has received in recent years, little is known about the characteristics of the activities that firms call "research and development." This lack of information has been a hindrance to progress in this area, since, without a reasonable amount of information on this score, it is difficult to formulate or evaluate models relating R and D expenditures to other economic variables. In an attempt to shed additional light on this subject, we studied the characteristics of the R and D programs carried out in 1964 by twenty-two major

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[1] In 1966, industry alone performed about $15 billion of research and development. For a discussion of the growth of R and D expenditures in the postwar (and earlier) period, see Edwin Mansfield, *The Economics of Technological Change* (Norton, 1968), Chaps. 3 and 6.

[2] Edwin Mansfield and Michael Hamburger, "A Study of Research and Development in the Chemical and Petroleum Industries" (unpublished paper, 1968). Only a few of our results can be presented in the time available here.

firms in the chemical and petroleum industries—two industries that are very important performers of R and D.

The results indicate at least four things. First, they show that the bulk of the R and D projects carried out by these firms is relatively safe from a technical point of view. In practically all firms in our sample, most of the projects were regarded as having better than a fifty-fifty chance of technical success.[3] Second, only a small percentage of the money spent by these firms goes for basic research. On the average 9 percent of their R and D expenditures were devoted to basic research, 45 percent were devoted to applied research, and 46 percent were devoted to development.[4] Third, most of the R and D projects are expected to be quite profitable if they are technically successful, the median expected rate of return for a firm's projects generally being about 30 percent.[5] Fourth, the bulk of the R and D projects are expected to be finished and to have an effect on profits in five years or less.

These findings seem to support the hypothesis advanced by Hamburger [6] and others, that the bulk of the R and D carried out by large corporations is relatively safe and aimed at fairly modest advances in the state of the art. Models or policies based on the popular supposition that the bulk of the research and development in the industrial laboratory is very risky, far-out work aimed at really major inventions are likely to be misconceived and misleading. This is an important point, both for analysis and policy.

The results also indicate that, in each industry, there is a great deal of interfirm diversity in the characteristics of the R and D programs.[7] For example, some chemical firms devote 18 percent of their R and D expenditures to basic research, whereas others devote none. Some chemical firms spend $47,000 on R and D per scientist and engineer, whereas others spend $25,000. The median expected rate of return is about 40 percent for some chemical firms and less than 20 percent for others. The median length of time to completion is about two years for some chemical firms and about four years for others. Only in the

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[3] Of course, it might be objected that these estimates are biased upward because the R and D personnel are trying to sell projects. But we have data from related studies for some firms that allow us to compare estimated probabilities of success with actual probabilities of success. The results indicate that, although there may be some upward bias, it is not very large on the average. For example, in one firm, the average estimated probability of technical success was about 80 percent whereas the actual percentage of projects that were technically successful was about 75. In another firm, these figures were both about 80. Of course, projects that were not carried out as intended. or that were dropped for commercial reasons, were omitted to provide a fair comparison. A project is defined to be a technical success if it attains its technical objectives in the budgeted time and within the budgeted cost; otherwise, it is defined to be a technical failure. Of course, such a simple classification can be misleading. For example a project may not attain its technical objective but it may nonetheless result in very valuable information. Despite these disadvantages, it seemed worthwhile to gather data concerning the estimated probability of technical success, since this estimate is commonly used by firms as a rough measure of the riskiness of a project from a technical angle, and it is likely to be as good a measure of this sort as one can obtain in a survey.

[4] We used the National Science Foundation's definitions of basic research, applied research, and development. For example, see *Basic Research, Applied Research, and Development in Industry, 1962* (NSF, 1965).

[5] Note that these estimates are conditional on the project's being technically successful. The unconditional estimates would, of course be lower.

[6] Daniel Hamberg, "Invention in the Industrial Research Laboratory," *J.P.E.*, Apr. 1963.

[7] Of course, some of this variation could be due to differences among firms in the interpretation of the definitions of research and development, basic research, applied research, development, scientist, engineer, and so on. We questioned a number of the respondents to be sure that they were fully aware of the definitions and that they interpreted them properly. We could find no evidence that they were interpreting the definitions in significantly different ways.

case of the median estimated probability of technical success of projects is the interfirm variation in each industry relatively small. In all but three firms it lies between 50 and 62 percent in chemicals; in all firms, it lies between 70 and 83 percent in petroleum.[8]

III. SIZE OF FIRM AND CHARACTERISTICS OF R AND D PROGRAMS

Some of this variation among firms in the characteristics of their R and D programs can of course be explained by differences in size of firm.[9] In particular, one would suppose that the largest firms would devote a larger proportion of their R and D expenditures to more basic, more risky, and longer-term R and D projects than their smaller competitors. Our data for the chemical and petroleum industries bear out these hypotheses. For example, consider a chemical firm with sales that are one-tenth of the sales of the largest firm in the sample. Such a firm devotes to basic research a percentage of total R and D expenditures that is about one-third of the percentage devoted by the largest firm in the sample. Similarly, such a firm has a median probability of technical success that is about 20 percent higher, and a median length of time to completion that is about 15 percent lower, than the largest firm in the sample.

However, it is very important to note that, although the largest chemical firm in the sample differs significantly from the relatively small firms in these respects, the difference between the largest firm and the average firm that is about one-half of its size, is generally small, if it exists at all. With regard to the percentage of total R and D expenditures devoted to basic research, the difference is only a couple of percentage points. With regard to the median probability of technical success and the median length of time to completion, the median probability is higher (not lower), and the median length of time is lower (not higher), in the largest firm in the sample than in a firm that is one-half its size. The results are much the same for the petroleum firms. Only in the case of the median length of time to completion is there a substantial difference between the largest petroleum firm in the sample and the average firm of about one-half its size.

Thus, the results indicate that firm size is associated with a substantial amount of the interfirm variation in these characteristics. They also indicate that, although the differences between the largest firms in the sample and the relatively small firms are sometimes quite large, the differences between the largest firms and firms of one-half their size are seldom large, if they exist at all. To the extent that the sample is trustworthy, the results suggest that, in these industries at least, firms as large as the largest firms in the sample are not re-

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[8] Note that we are concerned here only with the technical risks. Since technical success by no means insures commercial success, the total risks are considerably greater than indicated by the probabilities of technical success. But this does not alter the fact that the bulk of the projects tends to be relatively safe from a technical angle. For similar data for a large electrical equipment manufacturer, see Edwin Mansfield, *Industrial Research and Technological Innovation* (Norton, for the Cowles Foundation for Research in Economics, 1968), Chap. 3.

[9] For studies of the effect of firm size on total R & D expenditures, see Edwin Mansfield, *Industrial Research and Technological Innovation, op. cit.;* and Richard Nelson, Merton Peck and Edward Kalachek, *Technology, Economic Growth, and Public Policy* (Brookings, 1967).

quired to insure that the existing amount of R and D of a more basic, risky, and long-term nature is carried out. Firms that are about one-half as large as the largest firms in the sample invest about the same percent of their R and D budget in more basic, risky, and long-term projects as do the largest firms in the sample. This too is an important point.[10]

IV. THE COSTS OF DEVELOPMENT

Let's turn from the characteristics of a firm's R and D program to the characteristics of an individual R and D project. An important, and extremely difficult, question is: what determines how much it costs to develop a new product? Some work bearing on this question is being carried out by Jerome Schnee,[11] who has obtained detailed data concerning about sixty development projects conducted by one major ethical drug firm between 1950 and 1967. Specifically, he has information concerning the costs of clinical testing and product formulation for each drug, and he is interested in determining whether it is possible to explain a reasonable amount of the variation among drugs in these costs by the characteristics of the product being developed, the development strategy, and so on.[12]

Three types of development projects are treated separately, since the nature of the development work is quite different in each type. These three categories are development projects that are aimed at (1) new chemical entities, (2) compounded products, and (3) alternate dosage forms. In each category, it is hypothesized that the costs depend on the number of dosage forms (tablets, oral liquid, nasal spray, etc.) being developed, which dosage forms are being developed, the therapeutic classification of the drug, and the spectrum of the drug's activity.[13] Also it is hypothesized that there has been an upward trend in costs during this period, due to inflation and changes (partly reflecting FDA regulation) in development procedures.[14] In

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[10] It is important to note that we do not have complete data for all of the largest firms in these industries. Although there is no obvious bias in one direction or the other, the results pertain to only a sample of the firms, and it is always possible that this sample is unrepresentative in important ways. However, a reasonably large proportion of the firms are included, over 50 percent of the chemical firms with 1963 sales of $100 millions or more and over 50 percent of the petroleum firms with 1963 sales of $500 millions or more being included. Of course, nothing can be deducted from these results concerning other industries. Whether or not the same kind of results hold for other industries can only be determined by obtaining this sort of data for them.

[11] Schnee is working on a doctoral dissertation at the University of Pennsylvania. The results described in this section pertain to only part of his work and are tentative, since the project is still under way. The only other econometric studies of the determinants of development costs (of which I am aware) have been carried out at the RAND Corporation and pertain only to military development. Unfortunately, many of them are classified.

[12] Although the costs of clinical testing of the compound and of product formulation are important parts of the total R and D costs pertaining to a drug, they are not all of the R and D costs associated with a particular drug. There are large costs that are usually incurred before the right compound is found. This should be borne in mind in interpreting the results.

[13] Dummy variables are used to represent which dosage forms are being developed and the therapeutic classification of the drug. The "spectrum of activity" refers to the range of a drug's biological activity and is measured by a dummy variable showing whether the drug is being developed for more than one therapeutic market. Some of these variables were omitted in particular categories. Among new chemical entities, the variable representing which dosage form was being developed was omitted because it seemed much less important than the number of dosage forms and to conserve degrees of freedom. Among alternate dosage forms, the spectrum of activity was omitted since they all were developed for a single market. One would expect that, other things equal, development cost would increase with the number of dosage forms and with the extent of the spectrum of activity. Also, some types of dosage forms are more expensive than others to develop.

[14] The calendar year is used as an independent variable. With regard to the FDA, 1962 amendments to the Food, Drug and Cosmetic Act strengthened the regulatory authority of the FDA over the clinical testing of new drugs and outlined minimum requirements for studies to demonstrate the efficacy and safety of a new drug.

addition, it is hypothesized that the costs will depend on the priority of the project and on whether or not parallel development efforts are employed. Finally, a variable similar to that used by Peck and Scherer [15] and Marschak, Glennan and Summers [16] to measure the extent of the advance in the state of the art was also included, more or less as an experiment.[17]

In each category, a regression based on these variables can explain a substantial portion of the observed variation in costs, R^2 being 0.93 for new chemical entities, 0.67 for compounded products and 0.90 for alternate dosage forms. In part, of course, these high correlations may be due to the fact that the data pertain to only one firm. Not all of the independent variables turn out to be statistically significant. Among new chemical entities, the number of dosage forms, the priority attached to the project, and calendar year are significant variables. Among compounded products, the spectrum of activity, the state of the art advance, calendar year, the therapeutic classification, and the use of parallel development efforts are significant variables. Among alternate dosage forms, the type and number of dosage forms, the therapeutic classification, the state of the art advance, and calendar year are significant variables.

The results concerning the effects on costs of using parallel development efforts are quite interesting. Based on the theory underlying the use of parallel efforts, one would expect that parallel efforts would be more likely to reduce costs for new chemical entities than for compounded products or new dosage forms, since the uncertainty as to the optimal approach tends to be much greater in the former case. The results are consistent with this hypothesis, the estimated effect of parallel efforts being to increase costs among alternate dosage forms and compounded products and to decrease costs among new chemical entities. However, the effect of this variable is statistically significant only among compounded products.[18]

V. DIFFUSION OF THE RESULTS OF RESEARCH AND DEVELOPMENT

The results of industrial R and D, to be economically important, must be accepted and used. The final study I shall discuss is concerned with this diffusion process. Continuing along lines set forth in previous studies, I have carried out an investigation of the diffusion of numerical control in the tool and die industry, the purpose

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[15] Merton Peck and F. M. Scherer, *The Weapons Acquisition Process* (Harvard, 1962).

[16] Thomas Marschak, Thomas Glennan, and Robert Summers, *Strategy for R and D* (Springer-Verlag, 1967).

[17] This variable represented a rough judgment of scientists and project managers familiar with these drugs. Essentially, the procedure used to obtain these judgments was like that used by Summers and Peck and Scherer. Obviously this variable is crude, which may help to explain why it is not always statistically significant. One would expect, of course, that projects attempting large advances in the state of the art would cost more than less ambitious projects. Dummy variables are used to indicate whether parallel development efforts were used and whether the project was given high priority by top management. Data concerning these variables could be obtained from the firm's records. On a priori grounds, it is difficult to predict the effect of the priority variable since costs tend to be high if there is a great emphasis on time reduction and also if there is very little emphasis on time reduction. The results, where significant, suggest that high priority projects are cheaper. The expected effects of the use of parallel development efforts are discussed below.

[18] For discussion of the effect of parallel development efforts, see Burton Klein, "The Decision Making Problem in Development," *The Rate and Direction of Inventive Activity* (N.B.E.R., 1962); Richard Nelson, "Uncertainty, Learning, and the Economics of Parallel Research and Development Efforts," *Rev. of Econ. and Statis.*, 1961; and Marschak's work in Marschak, Glennan, and Summers, *op. cit.*

being to see how rapidly this innovation is spreading, the kinds of firms that are relatively quick to adopt it, the reasons given by firms for not using it, and the opinions of firm owners concerning the impact of the innovation on the structure of the industry.[19]

Numerical control—a way of operating machine tools by means of numerical instructions on tapes or cards—is certainly one of the most important innovations in this century. According to one leading research institute, numerical control "is the most significant new development in manufacturing technology since Henry Ford introduced the concept of the assembly line." [20] One of the industries most affected by this innovation is the tool and die industry. This industry is composed of a very large number of small firms, which means that we have the opportunity to study the diffusion of an innovation in an industry where there is very little concentration and where the organization of decision making in the firm is relatively simple. Other diffusion studies concerned with manufacturing have dealt chiefly with more concentrated industries.

To estimate the growth over time in the percent of tool and die firms using numerical control, a mail survey was carried out of the membership of the National Tool, Die, and Precision Machining Association.[21] The results indicate that less than 1 percent of the firms had begun using numerical control before 1961. By the beginning of 1966, the percentage had grown to 10 percent; and by the beginning of 1968, 20 percent of the firms were using numerical control. Allowing for differences in the profitability of using the innovation and the size of investment required, this innovation seems to be spreading more rapidly than innovations in other industries for which data are available. This is a noteworthy finding, because it supports the view that, all other things equal, innovations tend to spread more rapidly in less concentrated industries.[22]

VI. CHARACTERISTICS OF EARLY USERS

There are a good many reasons for expecting the larger tool and die firms to be quicker than the smaller ones to introduce numerical control. For example, the larger firms are more likely to have the financial resources to enable them to experiment, and they are more likely to have the technical know-how and the managerial qualities that are important in determining a firm's speed of response to a new technique.

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[19] Only a few of the findings can be presented in the available time. For a more complete discussion, see Edwin Mansfield, "Numerical Control in the Tool and Die Industry: The Diffusion of a Major Technological Innovation" (unpublished paper, 1968). For a review of the literature, see E. Mansfield, *The Economics of Technological Change, op. cit.*, Chap. 4.

[20] Illinois Institute of Technology Research Institute, *Technological Change: Its Impact on Metropolitan Chicago* (1964), p. 1.

[21] Note that all of the findings pertain only to the approximately 1,000 members of the Association, since the costs of extending the frame beyond the Association membership were out of the question. The Association members account for the bulk of the industry's output. Figures for all members of the Association for 1967 and 1968 were available from the Association's *Directory*, so these figures contain no sampling error. A carefully selected sample of firms was interviewed. The results, which are unbiased, are in close agreement with the results of the mail survey.

[22] This finding is in agreement with my previous results, which pertain to four other industries. They indicate that, holding constant the profitability of the innovation and the size of the investment, the rate of limitation tends to be greater in less concentrated industries. However, there were too few industries to be at all sure the relationship was not due to chance. See E. Mansfield, *Industrial Research and Technological Innovation, op. cit.*, Chap. 7.

This hypothesis is borne out by my results. The median employment of firms using numerical control at the beginning of 1968 was about seventy, while the median employment of nonusers was about twenty-five. Moreover, among the users of numerical control, there is a significant inverse relationship between the size of a firm and the year when it began using numerical control.[23]

Other variables that would be expected to influence whether or not a firm adopted numerical control before 1968 is the education and age of the firm's president. Better educated entrepreneurs are likely to be in a better position to understand the issues regarding numerical control, to have the flexibility of mind to use it, and to be in contact with technical and university centers and the relevant literature. Younger entrepreneurs would be more likely to make the break with the past, their emotional attachments to old skills and old technology being weaker and their willingness to take risks probably being greater than their older rivals. The data are consistent with these hypotheses. Most of the users (for which we have data) are college graduates while most of the nonusers finished high school or less; the median age of the users was about forty-eight while the median age of the nonusers was about fifty-five. However, when a multiple regression is run (age and education being independent variables, the dependent variable being a dummy variable showing whether or not a firm used numerical control before 1968), the effect of education is statistically significant, but the effect of age is not.

A carefully selected sample of twenty-eight firms without numerical control was interviewed to determine why they were not using it. Practically all claimed that it would not be profitable for them at present, the primary reason being that they do one-of-a-kind work. (About 20 percent of the firms planned to begin using it in the next year or two.) However, about 30 percent of the managers, by their own account, had important gaps in their knowledge and understanding of numerical control. About 10 percent of the firm owners, being close to retirement, had decided to stick with conventional methods until they retired—or until they were put out of business. Judging by the interviews and other evidence, the diffusion process seems to have been slowed perceptibly by misunderstanding of the innovation and resistance to changes.[24]

VIII. CONCLUSION

In conclusion, organized scientific and inventive activity is a relatively new, and very important, factor in the modern economy. This paper has presented a brief summary of the results of several empirical studies of industrial research and development, these studies focusing on its characteristics, cost, and diffusion. Some of the principal findings are the following: First, the bulk of the R and D projects carried out

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[23] To some extent, this relationship is probably due to a difference between the growth rates of users and nonusers of numerical control, users probably having had higher growth rates. But this can explain only part of the relationship, there being evidence that, when they began using numerical control, the early users tended to be larger than the nonusers. Of course, the fact that the larger firms tend to be quicker than the small firms to begin using an innovation does not contradict in any way the finding that the rate of innovation tends to be faster in less concentrated industries.

[24] Nonetheless, as noted in Section V, the imitation process seems to be going on relatively rapidly. Thus, these drags on the rate of imitation may be less important than in other cases for which we have data.

by a sample of major chemical and petroleum firms seems relatively safe from a technical point of view, most projects having better than a fifty-fifty chance of technical success. This supports the hypothesis advanced by some economists that the bulk of the R and D carried out by large corporations is relatively safe and aimed at fairly modest advances in the state of the art.

Second, the largest firms in the sample devote a larger proportion of their R and D expenditures to more basic, more risky, and longer-term projects than their smaller competitors. However, although the differences between the largest firms in the sample and relatively small firms are sometimes quite large, the difference between the largest firms and firms of one-half their size are seldom large, if they exist at all. To the extent that the sample is trustworthy, the results suggest that, in these industries at least, firms as large as the largest firms in the sample are not required to insure that the existing amount of R and D of a more basic, risky, and long-term nature is carried out. Firms that are one-half as large as the largest firms in the sample invest about the same percent of their R and D budget in more basic, risky, and long-term projects as do the largest firms in the sample.

Third, a detailed study of about sixty development projects carried out in a large ethical drug firms indicates that the cost of developing a particular product is related to the therapeutic classification of the product, its spectrum of activity, and the number and type of dosage forms. It is also related to the priority attached to the development project and to whether or not parallel development efforts are used, at least in some product categories. This seems to be the only econometric study of the determinants of development costs in the civilian economy that has been carried out to date. Much more work is needed.

Fourth, a study of the diffusion of numerical control in the tool and die industry indicates that, allowing for differences in profitability and size of investment, this innovation seems to be spreading more rapidly than innovation in other industries for which data are available. This finding tends to support the hypothesis that innovations generally spread more rapidly in less concentrated industries. The early users of numerical control tend to be the larger tool and die firms. Also, whether or not a firm is using numerical control seems to be related to the age and eduaction of the firm's owner; but when both age and education are introduced simultaneously, only the effect of education is significant.

Section IV—International Aspects of United States R&D Funding

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"Science Indicators New Report Finds U.S. Performance Weakening"

By Philip M. Boffey

From *Science*, Mar. 12, 1976, p. 1031.

American leadership in science and technology appears to be diminishing by most available indicators, according to data in a cautiously worded report just issued by the National Science Board, the policy-making body of the National Science Foundation.

The report, entitled "Science Indicators—1974," was transmitted to Congress by President Ford on 23 February. It is the board's seventh annual report and the second to present measurements of the strengths and weaknesses of science and technology in the United States. The indicators reflect a varied mass of data, ranging from employment statistics to patent awards to literature citations and trade balances. By some measures, the United States has improved its performance in absolute terms in recent years, but other countries have improved even more, thus reducing the American lead. In other cases, the American performance has deteriorated in absolute terms.

The report resolutely refuses to reach any overall conclusion as to whether American science is healthy or weak and whether one should be content or alarmed about the trends that it documents. Staffers who had prepared the predecessor report, "Science Indicators—1972," had attempted to include a series of conclusions and recommendations in that report. But the material was excised because of opposition from the National Science Board and the Office of Management and Budget which felt that the indicators were not adequate to measure the entire scientific enterprise and that even the limited indicators available were often difficult to interpret. So this time there was not even a serious attempt to tease a general conclusion from the data presented.

Nevertheless, for what it's worth, the bulk of the indicators that are used to compare the United States with other countries appear to be headed downward. This is true both of the indicators that measure the resources being put into research and development—such as money and manpower—and the indicators that measure the results coming out of a nation's research establishment, such as publications. Nobel prizes, patents, innovations, and productivity. Only two major output indicators—international exchange of technical "know-how" and balance of trade in research-intensive products—show improvement in the U.S. position.

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The indicators provide new insight on the importance of basic research to technological innovation, and on the relation between the size of an industrial firm and its ability to innovate. They also reveal that the American public, far from being disenchanted with science and technology, has actually grown more supportive in recent years (see box, p. 1032).

Where possible, the performance of the indicators is traced over a decade and a half, from 1960 through 1974. Like its predecessor report, the new report deals primarily with the resources put into R & D, since these are relatively easy to measure. But it also sets forth new measures of research "outcomes," some of which were developed especially for this analysis, and it extends the coverage of some indicators that were used in the previous report.

Virtually every section of the report is hedged with caveats warning about weaknesses in the data or difficulties in its interpretation. But the general message of the figures seems to be that, while the United States is still ahead by many measures, its lead is being eroded.

The downtrend shows up dramatically, for example, in a study of technological innovation that was conducted specifically for this report by an outside contractor, Gellman Research Associates, Inc. The study investigated some 500 major new products or processes brought into commercial use over the past two decades. The list included such innovations as nuclear reactors, oral contraceptives, integrated circuits, lasers, and weather satellites. Although the National Science Board concludes that the United States leads other nations "by a wide margin" as an innovator, that lead has diminished steadily and sharply over the past two decades. In the late 1950's, the United States produced 82 percent of the major innovations, but by the mid-1960's it accounted for only 55 percent. A slight upturn subsequently in our relative standing does not represent any increase in American innovation but rather a decline in British innovations.

What's more, there was a change in the nature of the innovations. The proportion of American innovations rated as "radical breakthroughs" declined nearly 50 percent between 1953–59 and 1967–73, while those rated merely as "major technological advances" doubled.

By several other measures of scientific "output," the U.S. lead also appears to be deteriorating. Thus, the United States was the largest producer of the scientific literature sampled in the 1965–73 period in all fields except chemistry and mathematics, where it was second to the Soviet Union. But in recent years, U.S. publications in chemistry, engineering, and physics have declined slightly in both absolute and relative terms, a trend which the report suggests may be linked to decreases in funding for those fields.

As for the quality of these publications, the report notes that a study of citations in the 1973 literature placed the United States first or tied for first in each of eight fields. Whether that finding has much meaning is a subject of dispute. The general theory behind citation analysis is that the most significant scientific articles will tend to be "cited" most often by subsequent authors, and that one can therefore measure the significance of a nation's scientific literature by constructing a citation index. However, one National Science Board member—Saunders MacLane, University of Chicago mathematician—argues in supple-

mentary comments that such an index may underestimate the Russian literature (few Westerners read it or cite it) and the French literature (a "small-scale, high quality effort" that traditionally keeps citations to a minimum for lack of journal space). Whatever the merits of the index, it covers just 1 year and gives no indication of trends.

However, another measure of quality and importance—the Nobel prize—suggests a slight decline in American dominance. In the 1971–74 period, the United States received 56 percent of the awards in physics, 57 percent in chemistry, and 44 percent in physiology and medicine—a smaller fraction in each category than was received in the 1951–60 period.

Two other measures of scientific "output" are also headed down. The "patent balance"—a measure of the success of American inventors in winning foreign patents as compared to the success of foreigners in winning American patents—remains favorable for the United States, but there was a sharp 30 percent drop in the balance between 1966 and 1973. The report suggests gloomily that "the number of patentable ideas of international merit has been growing at a greater rate in other countries than in the United States." Similarly, the level of productivity—which is affected, in part, by R & D—remains high and continues to go up in this country, but productivity gains were much larger in four other countries, with the result that the American lead "diminished significantly."

Only two of the major output indicators showed an improvement in the American performance compared with that of other nations. The United States had an increasing positive balance of payments from the sale of technical "know-how" (patents, licenses, and manufacturing rights) over the 1960–73 period, with four to five times more "know-how" sold to other nations than purchased from them. And the United States had a large, favorable balance of trade in commodities produced by "R & D-intensive" industries; the balance doubled between 1970 and 1974 alone.

As for the input side, two key indicators were down. The fraction of the gross national product spent for R & D has declined steadily over the last decade in the United States, while growing substantially in the Soviet Union, West Germany, and Japan. By 1974, the Soviets were spending 3.1 percent of GNP for R & D, the West Germans 2.4 percent, and the Americans 2.4 percent, although comparisons with the Soviet Union are "particularly hazardous" because of different accounting methods. Similarly, the number of scientists and engineers engaged in R & D per 10,000 population declined in the United States after 1969 but continued to grow in all other countries studied.

In addition to the international comparisons, the report presents indicators relating to R & D resources, basic research, industrial R & D, manpower, and public attitudes toward science and technology.

From the viewpoint of basic scientists, perhaps the most gratifying finding is that "basic research contributes increasingly to technological innovation, as reflected by the growing number of citations to research in patents associated with major advances in technology." That conclusion was reached in a specially commissioned study of the patent documentation associated with 179 major technical advances which occurred in the United States between 1950 and 1973. The special stud-

also found that most of the research cited in patents is now performed in the universities, whereas in the 1950's industry had been the prime source of such research.

A new feature of the year's report was the establishment of "industrial R & D and innovation" as a major indicator category. The report found that industrial R & D is concentrated in a few industries and in a relatively small number of companies within those industries. Just 31 companies accounted for more than 60 percent of all R & D expenditures by industry. Small firms (those with fewer than 1000 employees) produced the greatest number of major innovations during the 1953–59 and 1960–66 periods, but large manufacturing companies (those with 10,000 or more employees) led in innovations in the 1967–73 period.

One of the most striking trends to emerge from virtually every chart and table in the report is that federal support of science and technology has either leveled off or headed downward in most categories when measured in constant dollars (dollars adjusted for inflation). There is also evidence that this has affected research "output." Thus publications by university-based mathematicians and engineers slackened 2 years after federal expenditures for those fields were cut. Whether it matters if the United States maintains a lead over its international rivals in *all* fields of science is a question that is neither addressed nor answered by the National Science Board. But the Ford Administration's budget experts are said to have been concerned about some of the downtrends documented in the report. One well-placed NSF official claims the report was a key factor in winning a big budget boost for basic research in the Administration's proposed budget for fiscal year 1977.

"Global Productivity Challenges Push U.S. Into CAD/CAM Era"

By Mat Heyman

From *Professional Engineering,* September 1975.

Slowly, but not necessarily surely, the United States seems to be inching toward an era of computer assisted, almost totally automated manufacturing. The computer revolution, which continues to unfold in the shape of smaller, more efficient, more capable, and cheaper minicomputers, microcomputers, microprocessors, and microcontrollers has the potential to make the greatest impact on American manufacturing processes since all those Model T's rolled off Henry Ford's assembly line.

Yet there has been grave concern expressed of late in engineering, industrial management, and government circles that U.S. efforts in such advanced technology areas as computer-assisted manufacturing are falling far short of the pace that worldwide economic facts of life require. A growing number of private and public assessments of recent international manufacturing technology developments has been turning out some shocking facts and figures about foreign plans in the field of computer-assisted manufacturing.

When coupled with the obvious need to maintain this country's balance of trade in the world marketplace and improve the current lowly U.S. position on the global productivity totem pole, these foreign manufacturing initiatives appear to spell trouble for future American economic health.

Still, the foreign efforts and the warning signals have failed to attract the attention of many of Washington's top policy makers. And that is precisely what worries those engineers, industry managers, and government people who have been able to document substantial government, industry, and university cooperation on a number of ambitious research, development, demonstration, and implementation programs in other countries—programs which dwarf anything this country is now embarked upon in terms of computer-assisted manufacturing.

The latest warning signal is about to be transmitted from the U.S. General Accounting Office. Examining manufacturing sector efforts to improve productivity both here and abroad, the GAO has surveyed a large sample of metalworking companies in the U.S. and has held discussions with more than 200 industrial, academic, governmental, and financial organizations. GAO staffers also took an in-depth look at efforts in the United Kingdom, West Germany, France, Italy, Norway, Denmark, Sweden, and Japan. Reviewing the broad spectrum of latest developments in the manufacturing area, the GAO paid particular attention to computer-assisted production techniques—frequently referred to as "CAD/CAM."

A term not widely used in the U.S., even among engineers who are involved with computers or with manufacturing, CAD/CAM (an acronym for computer-aided design/computer-aided manufacture) encompasses a broad range of manufacturing functions. Some of the important computer applications include: the basic design process, facilities planning, parts production, assembly, inspection and testing, materials handling, inventory control, and work scheduling.

POTENTIAL BENEFITS OF CAD/CAM

Potential benefits of CAD/CAM incorporation span the manufacturing spectrum from improved plant safety, to greater efficiency in using raw materials, improved product quality and uniformity, and a number of cost-cutting gains. For instance, the National Machine Tool Builders Association estimates that CAD/CAM could boost machine utilization by 600 percent. CAD/CAM may bring with it an estimated 90 percent reduction in unfinished parts on the job floor. While difficult to document, CAD/CAM may also result in production energy savings.

"Discrete parts" manufacture was the prime focus of the GAO study. It is widely accepted that small batch (fewer than 50 parts) production is the prime potential target for productivity improvement. Mass production techniques already accommodate high levels of automation.

While the GAO work has reaffirmed the contention that the U.S. has more of the most advanced manufacturing technology in place than its foreign competitors, the agency has become alarmed at the lack

of diffusion of these methods within American industry. It appears according to the GAO and a number of others who have studied the problem, that advanced technology such as CAD/CAM is presently developed and used for the most part by a very few very large U.S. firms that can afford the amount of engineering work and the large capital costs which accompany the new systems.

The U.S. has led the way in the development of both the numerically controlled (NC) machine tool and the computer—two fundamental components of the CAD/CAM concept—as well as a number of other technologies crucial to computer-assisted manufacturing. Yet, Fred J. Shafer, whose GAO team undertook the manufacturing technology study warns that "Other countries have recognized, adopted, adapted, and innovated with this technology, so that their level of CAD/CAM technology is at least equal to ours."

The scale of CAD/CAM development efforts overseas is impressive. Mr. Shafer notes that typical foreign programs "provide for distribution of technology information, demonstration projects, government subsidy of hardware acquisition, assistance in software development for specific company applications, and encouragement of cooperative systems development."

For example, Japan's Ministry of International Trade and Industry (MITI) is sponsoring a proposal to develop a totally automated batch manufacturing plant by the 1980's, GAO reports. Among the manufacturing processes to be automated are forging, heat treatment, welding, presswork, machining, inspection, assembly, and painting. That plant will be part of a national plan to hasten the usual time lag in planning and implementing new technologies. A cooperative program, the automated factory will be helped along by about $116.6 million in government funds, according to GAO research. MITI also is providing substantial aid in the development of the industrial robot industry.

In West Germany, the government has established a national plan for data processing. A five year effort expected to cost a whopping $757 million, part of the plan's goal is to accelerate the application of computers in design and manufacturing. GAO investigators report that East Germany is making an active effort to apply new technology in batch production, and that several of the "most modern operations" are already in place.

Both formal and informal productivity centers operate throughout Europe. Ironically, it was in response to U.S. urgings after World War II that many of these centers were originally established. Sixteen centers have joined to form the European Association of National Productivity Centers. Fourteen Asian countries make up the Asian Productivity Organization.

FRAGMENTED U.S. APPROACH

Foreign emphasis on disseminating information on potential computer applications in the manufacturing sector and cooperating to develop and demonstrate unproven systems is contrasted by a largely fragmented U.S. approach to the CAD/CAM challenge. GAO found the U.S. to be perhaps the only advanced nation in the free world

which lacked a coordinated national level effort to stimulate development of manufacturing technology in the commercial sector. While concentration of technical CAD/CAM know-how in large firms was recognized overseas as well as in the U.S., several other countries are making a methodical effort to aid small- and medium-sized companies through cooperative programs.

The articles in this issue of *PE* touch on some of the areas of American advances in CAD/CAM. At the same time, they underscore the still underutilized computer applications in manufacturing already available. Certainly the ongoing developments in computer technology and cost reduction described in the article by Earl C. Joseph deserve the characterization "revolutionary." Great strides have been made in engineering design through interactive graphics. Yet Eli Glazer points out that "Many engineers who have worked with computers as pure electronic data processing (EDP) devices may be unaware of the 'interactive' uses of a computer, which are central to its use as an efficient design aid."

Even the available technology of the numerically controlled machine tool is vastly underutilized in the U.S. (The basic NC operation involves an electronic control mechanism which uses numerical data from a punched tape or a computer to control a machine tool's function.) While the GAO survey of metalworking companies revealed that NC was the only automation application used to any "measurable degree," a threadbare 17 percent of the respondents indicated they had one or more NC machine tools in operation.

Further evidence of the relatively minor NC impact on American manufacturing operations is the small size of the NC machine tool manufacturing industry. Overseas competition in this area is increasing, with developing countries getting into the act. According to George Putnam, technical vice president of the Numerical Control Society, "The Indians are turning out sophisticated high quality machine tools" and may "swallow up a good part of the market."

In answering the question "Is this country falling behind in automation?" from a Congressional subcommittee looking into U.S. research and development programs and policies, Assistant Secretary of Commerce for Science and Technology Dr. Betsy Ancker-Johnson acknowledged: "It is true in the area of numerically controlled tools. . . . We are not doing nearly as well, despite the fact that we certainly have the technology. This is one area in which we are falling behind and I think it will very much damage our productivity in the future and our competitiveness with our trading partners in the marketplace."

The Department of Defense, which was directly responsible for NC development largely through Air Force contracts with the Massachusetts Institute of Technology, is itself a major user of numerical control. But a GAO assessment of NC in the Defense Department determined that "Activities surveyed had no formal systems for identifying where numerical control could be economically used. They did not have adequate staffs to search out opportunities, did not make work mix studies, and usually bought NC equipment only when conventional equipment deteriorated or when new workloads were anticipated. Large amounts of equipment were planned for procurement, but very little was NC."

Some other disquieting GAO findings reflecting on the limited implementation of certain advanced automation technology include the fact that 51 percent of those metalworking firms surveyed used no computers at all. Fewer than one-fourth of all firms that did utilize computers have them on-site. And in most cases, computers have primarily found accounting and administrative rather than direct manufacturing applications.

FEDERAL GOVERNMENT CONTRIBUTIONS

The Federal government has made significant contributions to CAD/CAM technologies. Not only have defense and space demands led to the pioneering work on NC and computers, but remote control technology has been significantly advanced in keeping up with nuclear materials handling and space needs.

The Army, Navy and Air Force each have multi-million dollar manufacturing technology programs designed to cut costs on multi-billion dollar procurement bills. Computer-aided design and manufacturing projects are included in these programs, but contractor incentives to make greater use of CAD/CAM techniques are based largely on contract-by-contract considerations.

A recent Pentagon memorandum from Deputy Security of Defense William P. Clements points to the need for further, more substantial manufacturing technology developments within the military's scope of operations and may result in a more comprehensive look at CAD/CAM potentials. With an end goal of cutting defense materiel costs, Clements declared, "I am convinced there are numerous opportunities to obtain significant costs savings in the production of Defense materiel by increasing the application of state-of-the-art manufacturing techniques and by the development of new or improved manufacturing technology. For example, not only should we be making more effective use of numerically controlled machine tools and other new highly productive manufacturing processes but we should also be exploiting emerging technologies such as computer-aided manufacturing. . . ."

The National Bureau of Standards (NBS) has several computer-aided manufacturing projects underway, and has focused on industrial robots as a critical component in making the transition to CAM, NBS is the final stages of work for the Navy on low cost robots with specially designed hazardous materials handling capabilities. While specifically aimed at improving ordinance and radioactive materials handling, such a robot could be used by the private sector and could be an aid to industry—in complying with strict occupational safety and health rules, for example. Another NBS project still in the early stages will involve the use of industrial robots to load and unload machine tools. Dr. John Evans, in charge of the NBS developmental and automation control technology office, says that "The work in our laboratories with robots and integrated machine tools under computer control will offer us a test bed, a technical base, from which we can work cooperatively with other government agencies and with industry groups to develop interface standards . . ." NBS also cosponsors a number of conferences designed to explore and coordinate industry's CAD/CAM activities.

The advanced productivity and research technology program within the National Science Foundation (NSF) includes approximately $2.1 million worth of CAD/CAM-related projects. Reprogrammable automated assembly and inspection systems, automated parts manufacture, materials handling and storage automation, and computer based graphics systems to provide better linkage between designers and manufacturers are among the top priorities of the NSF program.

One of the very few examples of close government, industry, university, and independent research and development community cooperation in making improvements in computer assisted manufacturing technology is an ongoing project run by the U.S. Maritime Administration. The government's purchase of a Norweigian CAM system has formed the core of the "Research and Engineering for Automation and Productivity in Shipbuilding" (REAPS) program. REAPS is intended to go beyond development of a conceptual advanced manufacturing system and will concentrate on getting a CAM program implemented in the real manufacturing world.

In spite of these individual efforts at advancing the state of CAD/CAM technology, the Federal government role is a splintered one. There is no specific Federal policy to exploit the possible productivity-boosting impact of increased CAD/CAM development and adoption. Several other countries have such a policy and well-developed programs implement that goal.

While recent international trade statistics have put this country back in the black ink of the trading ledger, the high technology market is eyed with eagerness in other countries. Low technology exports have been running trade deficits. The tremendous outflow of dollars necessary to satisfy this country's energy needs mandates that the U.S. do whatever it can to hold onto or gain foreign markets.

The conventional wisdom has been to attribute higher productivity rates overseas to post World War II rebuilding. However, a productivity ranking list for 1960 through 1974 pegs the U.S. last among Japan, the Netherlands, Sweden, Belgium, Italy, France, West Germany, Switzerland, Canada, and the United Kingdom. Drastic labor cost hikes have moderated the impact of these gains in several countries, but the significance of the productivity statistics cannot be taken lightly.

Making the picture even dimmer, GAO reports that U.S. capital expenditures per worker were down by nearly 15 percent from 1967 through 1973. During the same period, West Germany's per worker investments rose about 133 percent, and Japan's were up about 70 percent.

NEW LOOK AT FEDERAL ROLE

While CAD/CAM seems destined to become a mainstay in the American manufacturing environment of the future regardless of Federal government efforts, the coordinated CAD/CAM activities in a number of other countries and the relatively poor U.S. productivity and capital investment position have led GAO and other groups to call for a fresh look at the Federal role in the manufacturing sector.

The Automation Research Council (ARC) has been among the most visible of these groups. With National Science Foundation subsidies, ARC has formulated a "National Research Plan for Automation." In addressing the problem of getting integrated CAD/CAM systems online in U.S. plants, the ARC suggests that ". . . it appears that the only viable solution for the United States which is available in a time frame competitive with other industrial countries lies in a joint government, industry, university effort." The ARC has recommended that such a joint program be initiated to the tune of $300 million over a seven-year stretch.

The path by which ARC arrives at a conclusion which would prompt more direct Federal government participation in furthering CAD/CAM is one taken by increasing numbers of CAD/CAM advocates. A large degree of intercompany coordination and cooperation is required to avoid re-inventing the wheel. Yet it is often claimed that such cooperation may not be possible under antitrust restrictions. Those antitrust hindrances must be dealt with through their clarification and/or relaxation. Otherwise work will have to be done by non-industry groups which may require government funding, says the council.

ARC's report states that "The combination of installation size, costs, and potential technological and/or financial risk along with the possible disruption of production which may occur may be such that a single company cannot undertake the contemplated development on its own. However, where the development in question has a larger potential benefit to many different companies in several different industries the aid of government in financing a trial installation to assess the technological risk and prove the resulting economic benefits would be quite appropriate." In addition, massive retraining to provide new manpower or to avoid a widespread labor dislocation may be necessary. ARC feels that "extracompany funding is absolutely necessary."

Standards setting efforts to avoid confusion and speed implementation while ensuring the compatibility of individual technological initiatives will be a major task. ARC argues that government funds to support administration and initial test facilities costs are needed.

Despite the hopes of CAD/CAM activists, the slumping productivity figures, and the reports from GAO and abroad about major programs in computer-aided manufacturing technology in other countries, there is little likelihood that the current limited Federal government CAD/CAM role will undergo any substantial change in the near future.

Proposals for Federal financial help in demonstration projects have failed to get a warm Washington welcome. When the argument for immediate CAD/CAM help is presented to a number of top Washington policymakers, the inevitable response is a reference to the potential of a revitalized Federal productivity commission or center. Indeed, as originally conceived in legislative bids by Senators Sam Nunn (D-Ga.) and Charles Percy (R-Ill.), a new center could very well have supported CAD/CAM research and funded limited demonstrations. That proposal, now winding its way through Congress, has been wa-

tered down so that any meaningful developmental work in CAD/CAM by a productivity center seems out of the question.

SERIOUS REVIEW NOWHERE IN SIGHT

A House Science and Technology subcommittee is considering a look at the state of automated manufacturing technology but has no hard and fast plans. Elsewhere in the Congress, the basic term "productivity" always raises the specter of labor-management conflicts, and a serious review of manufacturing technology is nowhere in sight. In an interview with *PE*, Assistant Secretary of Commerce for Science and Technology, Dr. Betsy Ancker-Johnson summed up her predictions about the chance for large infusions of Federal CAD/CAM demonstration money this way: "In the current economic climate? Absolutely zero."

Antitrust regulations are unlikely to be relaxed. In any event, there is no hard evidence to show that the Justic Department would frown upon inter-company CAD/CAM ventures. That such cooperation is possible is attested to by the success of a relatively new organization—Computer Aided Manufacturing-International, Inc., of CAM-1. The 47 member group aims to further the general development of CAM knowledge. While now made up primarily of large firms, small and medium-sized companies can benefit as well due to a CAM-1 policy of dedicating study results to "the public domain."

There is no doubt that the present tight capital situation is an obstacle to adoption of newer, more sophisticated manufacturing equipment. But the fate of tax revision proposals which would speed the rate of CAD/CAM utilization rests with the resolution of a much broader, ongoing dialogue between Treasury Secretary William Simon and Congressional leaders.

Those who contend that more attention must be paid to computer applications in manufacturing have their work cut out for them. Engineers, industry managers, and government personnel who are convinced that the CAD/CAM effort in the U.S. must be much larger will have to realistically address the economic, political, and social facts and implications in a forthright manner. CAD/CAM advocates, including engineers, must be able to justify CAD/CAM economics, possibly through a new look at accounting procedures which may not now provide for sufficient consideration of long-range benefits.

Even if hardware and software costs can be justified, the social costs must be weighed and included in the CAD/CAM equation. The labor-related difficulties may turn out to be massive. But those problems should be openly acknowledged and more completely assessed. While future shortages of certain skilled workers—such as metalworkers—are often assumed and are cited as a reason for going CAD/CAM, current economic realities must be faced. Through the first half of 1975, Bureau of Labor Statistics figures put the unemployment rate for metal craftsmen at 12.1 percent—which translates into 83,500 people without jobs. There is no guarantee of manpower shortages down the road.

As CAD/CAM technology grows, so does the need for standards coordination. The Federal government may play a coordinating func-

tion in the crucial standards arena, and perhaps act in a limited financial support role. Antitrust implications of cooperative inter- and intra-industry CAD/CAM plans may have to be more clearly spelled out by Washington. The state of uncertainty which now pervades industry certainly will not help CAD/CAM.

The odds are, however, that the Federal government is not likely to soon bestow any huge budget chunks on CAD/CAM—at least not until the implictions of current global manufacturing trends are more widely appreciated.

"Technology, International Competitions, and Economic Growth: Some Lessons and Perspectives"

By Keith Pavitt

From *World Politics*, January 1973.

With World War II and the explosion of the atomic bomb, the critical importance of organized research and development (R & D) to weapons development and to international relations became obvious to everyone. It created new problems in national policy, deriving essentially from the mobilization and the close involvement of university science and industrial technology with strategic and quasistrategic aims. And it changed the rules of the game in international relations by increasing, by many orders of magnitude, the costs of waging all-out war.[1]

The links between R & D on the one hand and economic competition and growth on the other have not been revealed in quite the same spectacular manner. But they have been growing steadily in importance since at least the end of the last century, starting with the foundation of the chemical and electrical industries and the first industrial laboratories. Even during the economically depressed 1930's, R & D in American industry increased by 12 percent annually in real terms. Between 1966 and 1971—a period that included three years of economic recession, government cutbacks in funding, and growing public hostility toward science and technology—industry-financed R & D in the United States still went up annually by about 5 percent in real terms.[2]

In other advanced countries, the growth of industry-financed R & D has been even more rapid. In the five non-Communist countries with the largest R & D efforts (France, Germany, Japan, the United Kingdom, and the United States), it increased by nearly 10 percent, in real terms, between 1961 and 1967. In 1961, industry-financed R & D had been at a level of just under three-quarters of total expenditures by these five governments on defense, nuclear, and space R & D. By 1967, the level was equal to these governmental R & D expenditures. In other words, in 1968 industrial competition and growth requirements replaced strategic competition and defense requirements as the biggest

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[1] See, for example, Robert Gilpin, *American Scientists and Nuclear Weapons Policy* (Princeton 1962); Raymond Aron, *The Great Debate* (New York 1964).

[2] James Worley, "Changing Direction of Research and Development Employment Among Firms," Table I, in Richard Nelson, ed., *The Rate and Direction of Inventive Activity* (Princeton 1962); National Science Foundation, *National Patterns of R & D Resources*, NSF 70–46 (Washington, D.C. 1971), 2.

single determinant of the allocation of scientific and technological resources in the non-Communist world.[3]

Certainly, in important sectors like energy, aerospace, and communications, the distinction between strategic and economic requirements is not very clear. Both are met by very similar technologies and often by the very same industrial firms. Moreover, the pace of development of one is dependent on the pace of development of the other. Thus, the successful efforts in defense technology that were mounted in the United States after 1940 benefitted considerably from American industry's strength in civilian technology, just as the more recent successful development of the "Viggen" fighter aircraft in Sweden no doubt benefitted from the technological sophistication and worldwide awareness of Swedish civilian industry. Conversely, American civilian technology in communications, nuclear energy, and aircraft benefitted from the government's forced-draft development of defense and space technology in the late 1950's and 1960's.

None of this belies the growth in importance of industry-financed civilian technology. This growth has posed new problems and created new challenges at a number of levels: to the industrial firm, where product innovation through R & D has become a major weapon in competition, but where the management, control, evaluation, and exploitation of R & D cannot be based on methods used in other fields of business activity;[4] to the discipline of economics where, in spite of strong resistance, considerable progress has been made in integrating science and technology into growth and trade theory;[5] to the national policymaker, who must design policies that will enable his country to take the best advantage of science and technology for both growth and competition; and to the diplomat, who, in the middle 1960's suddenly had to make himself familiar with such things as the braindrain, the technology gap, the multinational firm, and international technological cooperation.

In the following discussion, we shall concentrate on the last two aspects, namely, on how the growth of civilian science and technology has interacted with national policies and international relations in the 1960's, and on what lessons for the 1970's can be drawn from the experiences of various countries.

ECONOMIC GROWTH

There were two main strands in public policy concerning industrial science and technology in the 1960's. The first was to ensure the rapid diffusion and effective use of new technology so as to promote the growth in national productivity considered necessary for social stability and political acceptability. Originally there had been the mistaken belief that the contribution of technology to national growth could be increased simply by augmenting national expenditures on

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[3] Yvan Fabian, Allison Young and others, *R & D in OECD Member Countries: Trends and Objectives* AOECD, Paris 1971). This is the first comprehensive comparison of trends in R & D expenditures in the 1960's in the industrialized non-Communist countries.

[4] Keith Pavitt and Solomon Wald, *The Conditions for Success in Technological Innovation,* Part II, E (OECD, Paris 1971).

[5] See, for example, National Science Foundation, *A Review of the Relationship Between Research and Development and Economic Growth/Productivity* (Washington, D.C. 1971).

R & D. This ignored the fact that nationally developed technology amounted to only a small proportion of what was available, and that, at least in the industrialized Western countries, technology flows easily across national boundaries. Since the mid-1950's, the international diffusion of technology—through licensing agreements, direct foreign investment, and trade in producers' goods—has been increasing at a rate of more than 10 per cent a year.[6]

However, studies by economists have shown that a significant proportion of the growth in productivity levels in the advanced Western countries can be attributed to qualitative improvements in labor and capital—in other words, to education and the diffusion of new technology.[7] It is probably safe to assume that "technical progress" is built into the Western system of economic development and that, in addition to the steady growth of the world's industrial R & D, the continuous upgrading of skills and continued industrial investment have been key elements in the process.

The experience of the Eastern European countries, however, suggests that these elements—while necessary—are not sufficient. In these countries very high priority is given to R & D, to education, and to industrial investment. Yet such evidence as there is suggests that economic growth rates—even though comparable to those in Western countries—owe very little to qualitative improvements in capital and labor.[8] Moreover, there is ample evidence of lags in the application of specific technologies, such as computers.[9]

This state of affairs could be the result of a conscious policy of priorities: for example, the backwardness of Russian chemical technology might simply be a reflection of the relatively small importance attached to consumer goods where many plastics and synthetic fibres find their eventual use; and the general backwardness of civilian technology could be attributed to the high priority given to defense. It could also reflect inefficiencies in the exploitation of foreign-developed technologies: as the general level of technology, and specifically Russian needs, has become more sophisticated, the traditional policy of stripping down foreign machines and of reading foreign patents and blueprints has become an increasingly ineffective substitute for exchanges of "know-how" and close, person-to-person contacts. In addition, it could reflect the futility and inefficiency of centralized decision-making and on-paper planning of scientific and technological advance and application.

In this context, it is interesting to contrast the American and Russian experience in defense and civilian technologies. Faced with rising defense costs, the U.S. Government tried, in the 1960's, to plan a rapid advance in defense technology more effectively by making early choices

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[6] OECD, *Gaps in Technology: Analytical Report,* Book IV (Paris 1970).

[7] See, for example, Edward Dennison and Jean-Pierre Poullier, *Why Growth Rates Differ* (Washington, D.C. 1967) ; also fn. 5.

[8] The best general sources on Eastern European (mainly Russian) science and technology are: OECD, *Science Policy in the USSR* (Paris 1969) ; Radovan Richta, *Civilization at the Crossroads* (Prague 1968) ; Michael Boretsky, *Comparative Progress in Technology, Productivity, and Economic Efficiency: USSR versus USA,* and *The Technological Base of Soviet Military Power,* U.S. Congress, Joint Economic Committee (Washington, D.C. 1966 and 1970 respectively) ; Christopher Freeman and Alison Young, *The Research and Development Effort in Europe, North America and the Soviet Union* (OECD, Paris 1965) ; UNESCO, *Statistics on Research and Experimental Development Activities, 1967* (Paris 1970).

[9] Ivan Berenyi, "Computers in Eastern Europe," *Scientific American,* CCXXIII (October 1970).

among possible technological configurations on the basis of detailed paper analysis, thereby eliminating "duplication" in research and advanced development. The result was the F–111. In the U.S.S.R., competition among alternative designs has been actively encouraged with, it appears, good results, and the United States has reverted to a policy of "fly before buy." [10]

In the civilian sector, however, the reverse has been happening. In the USSR, lines of technical development to be pursued tend to be established centrally, and attempts are made to cut down the number of different types of producers' goods incorporating new technology; whereas in the West, technological initiatives are decentralized and competitive, and producers' goods are highly differentiated.[11] It seems that the U.S.R. still has to find the functional equivalent of the capitalists' oligopolistic competition in producers' goods for putting cost-effective technology to use.

The Russian authorities are well aware of these deficiencies and have taken measures to try to correct them. As a necessary prelude to a considerable increase in the importation of Western technology through direct agreements with Western firms, the U.S.S.R. has adhered to the Paris convention on the protection of industrial property. Most of these agreements have been with European and Japanese firms, a fact that has led to successful pressure by American firms on the U.S. Government to liberalize its regulations on the exportation of technology to Communist countries.[12]

It is doubtful that this infusion of Western technology will, in and of itself, make the issue of Russia's civilian tehnological resources more efficient. The U.S.S.R. has not demonstrated the Japanese ability to import foreign technology, to diffuse it rapidly and—with a combination of absorptive R & D, cheap but educated labor, and strong competition within a large home market—to improve it and to export the resulting products at competitive prices a few years later. This means that, when considering such agreements, Western firms probably do not fear that they would be creating Russian competition against themselves in a few years' time. But the U.S.S.R. is short of hard currency, which means that the terms that it can offer are not too generous. There may be growing possibilities for barter agreements, in which Western firms subcontract component manufacture and assembly operations and take advantage of the cheap but well-educated Eastern European labor force. However, insofar as Western firms are multi-national and anxious to retain full control over integrated, worldwide operations, they may be reluctant to license their technology in Eastern Europe for fear that Western European countries and Japan demand the same treatment as Eastern Europe.

Furthermore, the U.S. Government seems reluctant to liberalize the exportation of its computer technology to Eastern Europe com-

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[10] For an admission of the weaknesses of the previous U.S. system, and intentions for the future, see David Packard's speech to representatives of defense firms on August 11, 1974 (Department of Defense News Release No. 689–71).

[11] For a description of the characteristics of "research-intensive" industries in the West, see William Gruber and Raymond Vernon. "The Technology Factor in a World Trade Matrix, in Raymond Vernon, ed., *The Technology Factor in International Trade* (New York 1970) ; for a description of the USSR's "uniform-technology" policy, see Bruce Williams, *Technology Investment and Growth* (London 1967), 147.

[12] For a list of Russian purchases of Western plant and equipment, see Marshall I. Goldman, "More Heat in the Soviet Hothouse," *Harvard Business Review*, XLIX (July-August 1971), 15, exhibit IV.

pletely. But although the importance of computers cannot be denied, the combination of indigenous backwardness and the difficulty of getting access to foreign developments may have made the Russian authorities see in computers virtues and attractions that may dissipate quickly upon more intimate acquaintance. Great hopes seem to be placed in the Ninth Five-Year Plan on the use of computers and other paraphernalia of "advanced management" techniques to improve economic performance.[13] One proposal—to create a vast network of linked computer information systems in order to improve the central planning of the economy—appears to ignore not only the tremendous technical difficulties of such an undertaking, but also the not very reassuring Western experience to date with "management information system."[14]

The Russian authorities have, over the past few years, taken a series of measures intended to improve the results of their civilian R & D. In particular, they have tried to improve the links between industrial R & D and manufacturing operations, and to heighten the rewards for successful inventors. But little progress seems to have been made in creating competition among industrial firms, or in giving industrial firms autonomy in product development and product pricing. Perhaps one can understand the reason if one accepts Professor Burns' conclusion that technology is more successfully developed and applied in an "entrepreneur-centered" system than in a "management-centered system," and if one thinks of the consequences for power relationships within the U.S.S.R. of moving from the latter to the former:

In management-centered organizations the problems and tasks facing the concern as a whole are broken down into specialisms. Each individual pursues his task as something distinct from the real tasks of the organization. . . . "Somebody at the top" is responsible for seeing to its relevance. . . . Operations and working behaviour are governed by instructions and decisions issued by superiors. This command hierarchy is maintained by the implicit assumption that all knowledge about the situation of the firm and its tasks is, or should be, available only to the head of the firm. . . .

Entrepreneur-centered systems are adapted to unstable conditions, when problems and requirements for action arise which cannot be broken down and distributed among specialist roles within a closely defined hierarchy. Individuals have to perform their special tasks in the light of their knowledge of the tasks of the firm as a whole. Tasks lose much of their formal definition. . . . Interaction runs laterally as much as vertically. . . . Omniscience can no longer be imputed to the head of the concern.[15]

INTERNATIONAL COMPETITION

The second strand of public policy toward industrial science and technology has been to try to ensure that, because of the direct economic benefits involved, national technology be competitive in world

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[13] New York Times, February 15, 1971, p. 2.

[14] Alan Westin, "Information Technology and Public Decision-Making," in *Program on Technology and Society*, Harvard University, Sixth Annual Report, 1969–1970, 65.

[15] Tom Burns, *"The Innovative Process and the Organization of Industrial Science,"* in Main Speeches, European Industrial Research Management Association, Conference Papers, V (Paris 1967); cited in Pavitt and Wald (fn. 4), 61.

markets in at least some sectors. It has also been the policy to maintain or induce a strong effort in fundamental science, which in turn permits the monitoring of worldwide developments in science and technology. There has been another powerful motive, namely, to avoid complete technological dependence on foreign sources, and thereby to satisfy what Professor Vernon has described as "powerful psychic and social needs on the part of élite groups . . . including the desire to avoid a sense of dependence on outsiders." [16] Such an objective might sound trivial, and it has in fact led to some unrealistic and unsuccessful policies. But the history of 1776 and since shows that nations are made of the stuff that Professor Vernon describes.

This concern about foreign technological domination was widely expressed in Europe in the 1960's, when it was feared that the huge American lead in aerospace technology would spread to other technological sectors and that, through the medium of American multinational firms, Europe would become technologically depleted; in other words, that a technologically "unipolar" system would develop, which—even if it enabled Europe to be materially well off—would lead to the concentration of both R & D activities and high-level decisionmaking in the United States. But the most recent OECD statistics show that in fact quite the reverse has been happening. Throughout the 1960's, the relative weight of the United States in the Western World's science and technology was steadily decreasing. (See Tables 1 and 2.) By 1967, the U.S.A. still maintained considerably more R & D in the aerospace industry, mainly in response to the national defense and space programs; but in nuclear energy and in industry-financed R & D, Europe was undertaking about the same amount as the United States. Furthermore, throughout the 1960's, Europe steadily improved its trading position in high-technology products, and European firms continue to be active in most areas of advanced technology.

In other words, Europe appears to have responded very effectively to the "American challenge." But, in spite of the hopes of many, the challenge did not lead to a more concerted and integrated continent-wide response. Individual European countries have been able to do very well on their own in science and technology. Certainly, in the expensive scientific field of high-energy physics, successful cooperation is continuing in spite of budgetary difficulties and a small and unusual flurry of scientific nationalism from Germany. But in expensive sectors of technology, European cooperation has been a failure, mainly because of unwillingness to pay the political price to make such cooperation successful—namely, greater technolocigal interdependence and agreement on political objectives.[17]

Ironically, it is now in the United States that debate and discussion concerning a European and Japanese technological "challenge" is most animated. Whereas the Europeans were concerned about U.S. foreign investment, American concern is focused on the steady erosion of its trading position in high-technology products.[18] The same policy

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[16] Raymond Vernon, "The Multinational Enterprise: Power versus Sovereignty," *Foreign Affairs*, XLIX (July 1971).

[17] For a fuller discussion, see Pavitt, "Technology in Europe's Future." *Research Policy*, 1 (August 1972).

[18] See Michael Boretsky's paper, presented before the National Academy of Engineering, Symposium on Technology and International Trade, October 1970; also, Philip Boffey, "Technology and World Trade: Is There Cause for Alarm?" *Science*, CLXXII (April 2, 1971), 37–41.

measures are now being advocated here as in Europe in the 1960's: R & D subsidies to industry, mergers, and protection against foreign competition.[19] However, just as European perceptions of the American technological challenge were often exaggerated in the 1960's, and the response of European governments misplaced, so the European and Japanese challenge may today be exaggerated in the United States, and the proposed responses may be unproductive.

In particular, as has been argued by Nelson and Eads, it is unlikely that governmnet subsidies to specific, comercially-oriented technical ventures in industry will improve the technological competitiveness of American industry.[20] We have already noted the steady and continuing increase in industry's expenditure on R & D in all Western industrialized countries, including the United States; this suggests that industry is now well aware of the advantages of financing R & D activities. Furthermore, the experience of European countries in the 1960's suggests that it tends to remain to governments to finance projects which private sources do not consider worthy of support. Although there have been occasional successes, no government agency set up to support industrial R & D has made a commercially respectable profit. And it is interesting to note that Swiss industry, which ranks among the most technically sophisticated in the world, is strongly opposed to direct government-financing of industrial technology.[21]

What happened in the European aircraft industry shows very clearly that government money almost inevitably ends up in unprofitable technical ventures. In the 1950's and 1960's, European firms were facing much larger technical and market risks in developing civil aircraft than their American counterparts, given the latter's privileged access to a much larger military market and the close links between military and civilian technology. In consequence, private money went less readily into aircraft development in Europe than it did in the United States, and the substitution of European governments' money rarely enable the European firms to overcome their fundamental technical and market disadvantage. For example, even with considerable financial support from the British Government, the aero-engine firm Rolls-Royce went bankrupt trying to compete against engines whose prototype had already been developed in large part with U.S. Government money. However, the worm has turned and, ironically enough, it is now the areospace industry in the United States that is conjuring up a European challenge as a justification for greater government support.[22] But, as the figures in Table 3 show, this challenge is not realistic: aerospace is the one area where the R & D effort of American industry is overwhelmingly greater than that of European firms.

Another argument advanced to justify greater government support to the American aerospace industry is that the defense programs of the United States no longer give the impetus to civil aircraft development that they have given in the past, and that alternative methods of stimulating civil aviation techonlogy are therefore required.[23] But

[19] See "The U.S. Searches for a Realistic Trade Policy," *Business Week*, July 3, 1971.
[20] Richard Nelson, "World Leadership, the Technological Gap and National Policy," *Minerva*, IX (July 1971). George Eads and Richard Nelson, "Governmental Support of Advanced Civilian Technology: Power Reactors and the Supersonic Transport," *Public Policy*, XIX (Summer 1971).
[21] OECD, *Reviews of National Science Policy: Switzerland* (Paris 1971).
[22] See *Aviation Week and Space Technology*, May 31, 1971.
[23] *Ibid.*, June 21, 1971, p. 15.

although industry did, and obviously should, exploit in the civilian sector the forced-draft technological change induced by defense and space programs, it does not necessarily follow that the forced-draft can or should be maintained once defense and space programs have slowed down. It was such thinking that led the U.S. Government to be drawn into an expensive commitment to finance the SST. Given the European experience, the rejection by the U.S. Congress can be interpreted not as a vote against technological progress, nor as a triumph of ecology over economics, but as a refusal to begin the type of financing of civilian technology that leads to commercial failure.

The real problem lies elsewhere—in adjusting to the reduction in the demand for new aerospace technology that is the result of the relaxation in tension between the U.S.A. and the U.S.S.R., the end of the space race, changing patterns of requirements of the aircraft industry, and public boredom with and even hostility to technology spectaculars. It takes political temerity to let advanced-technology companies fail. Only Mr. Heath has done it so far with Rolls-Royce. As long as little effort is made by the government to alleviate the very real human costs of letting aerospace activity run down, each country's industry is bound to use spurious arguments—whether they be related to the balance of payments, maintaining a stake in advanced technology, resisting foreign challenges, or ensuring security of supply—to justify its continued existence.

Furthermore, it is very doubtful that "international cooperation" can be the solution to the problems of the aerospace industry. Given its economically depressed state, such "cooperation" will often be nothing more than a euphemism for each government trying to get other governments' money and markets for its own industry. Even if, as in the proposed United States-European cooperation on the Space Shuttle, the aerospace industries of various nations cooperate, share the work, and present joint proposals to a number of governments, very big difficulties will remain, given the commercial and/or strategic importance of certain parts of aerospace technology. Thus, while the United States will be very reluctant to become dependent on foreign technology, this will be precisely the objective of foreign countries—and often in the very areas which the U.S. Government and U.S. industry would least like to see in foreign hands. For this reason, proponents of transatlantic aerospace cooperation in general are likely to have considerable difficulties in agreeing when it comes to settling the details.[24] On the other hand, opponents of large-scale government commitments to aerospace are much more likely to be able to make common cause in order to try to avoid international agreements which, as the Anglo-French agreement on the Concorde shows, make large-scale programs even more difficult to stop, once started.

The fears of European nations and the United States about each other's technological challenge may be exaggerated and used to justify continued government support of defense-related industries, but Canada may have a real problem of foreign technological domination. Manufacturing industry—and particularly high-technology manufacturing industry—is mainly owned by U.S. firms. Technology and

[24] Burl Valentine, "Obstacles to Space Co-operation: Europe and The Post-Apollo Experience," *Research Policy,* I, No. 2.

the current Canadian concern for greater industrial independence are closely interwoven. The achievement of greater independence requires that Canadian industry develop specialized technological strengths which are competitive in world markets. And if it is true that United States firms are undertaking less R & D in Canada, Canadian scientists and engineers will have to depend increasingly on the existence of Canadian-owned firms in high-technology industries for finding productive employment.[25]

Under these circumstances, Canadian policy for science and technology has not unnaturally been a subject of intense discussion and debate over the past few years, and a good part of this discussion has been focussed on government machinery for formulating such a policy.[26] However, as Robert Gilpin has pointed out, formal government machinery cannot be a substitute for industrial entrepreneurship, nor for the close and informal relationships among industry, the universities, the civil service, and the banking community—these being the essential ingredients for the successful exploitation of technology in world markets.[27]

At a time when Canadians are intensely concerned about maintaining their independence, it may seem perverse to suggest that the most effective way to foster the above ingredients might well be to lower the external tariff, thereby increasing Canadian industry's exposure to worldwide competition. Yet the three small European countries which do have very strong civilian science and technology—the Netherlands, Sweden, and Switzerland—are all countries with low industrial tariffs.[28] Their situation must be compared to that of France, where very little in the way of commercial benefit came out of the flowering of French technical inventiveness at the turn of the century—possibly because of the heavy protection afforded to French industry and the inefficiency and sleepiness that this allowed. When, after 1958, French industry was exposed to foreign competition for the first time for nearly one hundred years, there was considerable concern that this would lead to foreign technological domination. Yet the most important and lasting feature of France's technological resurgence has not been the large-scale government-financed programs where prestige was often a more important consideration than commercial viability, but the very considerable increase in French industry's own expenditures on R & D International competition appears to sharpen the businessman's mind wonderfully, and it soon becomes evident to him that good technology and good science are essential components of commercial success.

THE INTERNATIONAL IMPERATIVE: SPECIALIZATION AND INTERDEPENDENCE

It will be abundantly clear from what has been said above that national policies for science and technology are very heavily influenced by the international framework within which they are undertaken.

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[25] Peter Meyboom, *Technological Innovation in Canada,* Economic Development Division, Department of Finance (Ottawa 1970).
[26] OECD, *Review of National Science Policy: Canada* (Paris 1970).
[27] Science Policy for What: The Uniqueness of the Canadian Situation," mimeo, Woodrow Wilson School of Public and International Affairs (Prineton 1971).
[28] See Pavitt and Wald (fn. 4), 52–56.

For the Western, advanced countries, this framework has offered not only opportunities for economic growth and commercial success in world markets; it has also imposed increasingly severe constraints on what national scientific and technological programs can expect to remain viable. It rarely makes economic sense to conduct R & D that has already been done in another country, precisely because there is more than enough interesting and useful R & D to be done, and because scientific and technological knowledge flows easily across national boundaries. And, assuming that the efficiency of R & D is higher when there is competition, international competition—through judgment of international peers for science, and through judgment of the international market for technology—is a convenient way to keep national science and technology efficient.

Thus, in science and technology as in trade, a case can be made in the industrially advanced countries for a policy of specialization within a framework of international competition and exchange. And, in science and technology as in trade, the case for internationalism can be made, not on the basis of traditional patterns of behavior, nor of some messianic intentions for the future, but on the basis of national self-interest in participating in such a system.

Events in the advanced Western countries have proven during the 1960's that the case for such specialization, competition, and exchange is a compelling one. It has been exceedingly difficult to sustain national programs that are second-best in civilian technology. The British have found this out in the aircraft industry, where the cost of developing and operating uncompetitive British designs has become very high. Similarly, the French Government decided to buy American-designed nuclear power reactors because—even after considerable public expense—the nationally developed system would have been too expensive to operate; now, certain agencies of the French Government are also having second thoughts about the large-scale government support being given to French technology in color television and computers.[29]

This international imperative will be felt in other countries in the 1970's. Germany was kept out of the strategic technologies after the second World War, and benefitted considerably from concentrating its resources on civilian technology. During the 1960's, however, the German Government appears to have been increasingly eager to buy its way into aerospace technology. But it is unlikely that Germany will be any more successful than Britain and France in maintaining a large, commercially viable R & D effort in this sector. Similarly, since the second World War, Japanese industry has been very successful in moving into new sectors of civilian technology through buying licenses from foreign firms which are unable to penetrate the heavily protected Japanese market by means of exports or direct investment. Under the strong pressures from foreign governments to open the Japanese market to foreign firms, it will become increasingly difficult for Japanese firms to obtain licenses from foreign firms on terms as favorable as they have been in the past. Even Japanese scientific and technological resources are likely to reach limits, and a more conscious policy of specialization will impose itself.[30]

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[29] See Robert Gilpin, "Technological Strategies and National Purpose," *Science*, CLXIX (July 31, 1971), 441–48. L. Stoleru, "Quand l'Etat se mele du marché," *Le Monde*, June 25, 1971.

[30] See Nicolas Jéquier, *Le Défi Industriel Japonais* (Lausanne 1970).

But it is perhaps in the United States that the imperative of specialization will make itself most keenly felt in the 1970's. As we have seen, industry-financed R & D in the United States is still increasing steadily; and, if only because of the close links between national capabilities in science and in technology, a case can be made for steady and growing government support for American academic research.[31] But the United States no longer exclusively carries the burden of making sure that the world has enough science and technology to solve its problems. The total amount of academic and industrial research being done is still increasing fast, even if the U.S.A. is making a relatively smaller contribution to the total. The objective sometimes expressed in the United States of being "second to none" in all sectors of science and technology is both costly and unnecessary. It makes economic sense for the U.S.A. to become relatively more dependent on the world system: in academic science through increased opportunities for foreign travel and study for American research workers; and in technology through increased licensing of foreign technology, foreign direct investment in the United States, and imports of producers' goods.

By comparison with the United States, most other countries live, and will continue to live, with a considerably greater degree of dependence on foreign science and technology—without their indigenous capabilities or their economic vigor being impaired. Furthemore, Germany saw a much more traumatc change in its scientific and technological ambitions as a result of the second World War, and Britain experienced a slower but equally fundamental change during the 1960's. This did not happen without some pain and some discord, but neither did it destroy either nation's scientific and technological strength.

THE ENVIRONMENT AND PUBLIC GOODS

A relatively new factor influencing national policies toward industrial R & D reflects, in one sense, the success that R & D has had in promoting economic growth. This is the growing concern about the effects of technological progress and economic growth on the physical and human environment. However. diametrically opposed conclusions are often drawn as to what the implications are. For some, technological progress and economic growth must be stopped if we are to prevent the depletion of raw materials on our "spaceship earth," or to avoid death through asphyxiation or overcrowding. For others, there exists an "ecological challenge" and/or a "social challenge," which requires a scientific and technological effort equivalent in scale and nature to the Apollo program. Both of these conclusions are probably wrong.

Certainly, if economic growth were no longer considered a desirable policy objective, the *raison d'etre* of about half the R & D undertaken in the world would be removed.[32] But although the opponents of economic growth may often be intelligent, articulate, and far-sighted, they are also a materially well-off minority. It is very doubtful indeed that their admonitions not to become as well off as they are will be listened to with much sympathy by economically less fortunate groups in both the advanced and less developed countries. And, though they may

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[31] For a detailed discussion of the links between national capabilities in academic science and in industrial technology, see Pavitt and Wald (fn. 4), Part III.

[32] See Fabian and others (fn. 3).

be intelligent and far-sighted, so was Thomas Malthus, who—because he left out of his calculations the possibilities of technological progress in agriculture—made some very erroneous predictions about the future of the world. Shaky predictions of doom may be made today because of the assumption that the relationship between production levels and pollution levels is necessarily positive and because of non-recognition of the effects of the price mechanism on the degree of recycling of raw materials and on technological progress which saves on their use.[33]

However, R & D will certainly be essential to effect a reduction in pollution and, if necessary, to economize on the use of raw materials. There is also the chance to remedy the justified criticism that, although science and technology have been made to respond in a very successful manner to the needs of individual consumers, of industrialists, and of the military, this has not been the case with regard to the growing needs for public goods and services like health, mass transportation, and urban development.

But does the answer to these problems lie simply in spending more money on R & D in these new social areas? Certainly, government expenditures in North American and Western Europe have been increasing rapidly, but in 1968 they nowhere amounted to more than 7 per cent of national expenditures on R & D, and in many cases to considerably less.[34] Perhaps more revealing is the fact that the United States has been spending a higher percentage of its GNP on Research and Development on health, pollution, and urban and social services than have Western European countries. Yet, such information as there is shows that, in Northern European countries at least, the standards of health care, urban amenity, and social service are higher than in the U.S.A.

This considerable variation across the Atlantic in the impact of socially oriented R & D [35] suggests either that science and technology cannot really help very much in these areas, or that there is difficulty in applying the science and technology. The practice of the industrial sector suggests that three ingredients are essential if science and technology are to be applied in a socially responsive manner: a need which is expressed in monetary terms; entrepreneurship; and money costs that reflect social costs. Insofar as the social needs are most acute among the poor, or more generally in urban regions, a significant transfer of resources may be required to ensure that money demand reflects social demand; entrepreneurship thrives on competition and freedom of entry, which may require the breaking up of traditional patterns of relationships between local political authorities and local suppliers and getting money costs to reflect social costs may mean the imposition of heavy penalties on those who cause pollution and congestion.

All these problems are fundamentally political—in other words, they involve moving resources, burdens, and power from one group in society to another. They cannot be conjured away by "technology assessment," "cost-benefit analysis," "social engineering," "system

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[33] For a fuller discussion, see Pavitt, "A European's View of the Environmental Crisis," Woodrow Wilson School of Public and International Affairs (Princeton 1971).

[34] See Fabian and others (fn. 3).

[35] With economically motivated R & D there are no similar variations in efficiency across countries. See Pavitt and Wald (fn. 4), Annex A.

analysis," or other social science equivalents of the "technological fix." Until they are solved, there may be very little that science and technology will do to help.

All this does not, of course. absolve policy-makers in the field of science from their responsibility to stimulate the R & D which could help solve these problems: for example, there is immediate need for fundamental research on ecosystems, for a concentrated effort to find cost-effective substitutes for persistent pesticides, and for stimulating the general state of the art in ground transportation technology. But it is very doubtful indeed that these new programs will lead to projects of the same scale and nature as the big defense and space projects of the 1950's and 1960's. For example, the development of the high-speed passenger train in Britain which, if successful, will revolutionize inter-city transportation there, has drawn heavily on aeronautical skills but will cost only about $15 million, including two prototype trains—far less, in other words, than development costs for a passenger aircrafts.[38] In fact, much of the R & D required will be rather mundane, decentalized, and form a part of wider tasks, such as government scientists and engineers drawing up industrial pollution standards and helping enforce them; industrial scientists and engineers designing and building equipment with an eye to an additional, environmental constraint; management experts finding relatively simple means of improving prenatal health care for the urban poor.

Unfortunately, there is the danger that government-supported, "socially oriented" R & D programs will become mainly a means of keeping high-energy physicists and aerospace engineers in jobs, or of avoiding politically painful actions as the enforcement of pollution standards, or of trying to find uses for clever and sophisticated technologies developed for other purposes. But the long-run viability and growth of R & D that responds effectively to social and environmental needs will ultimately depend on meeting the political conditions set out above. To the degree that they are met, past experience has shown the capacity of science and technology to respond rapidly to new demands. But predicting if and when they will be met is far more difficult than predicting the future course of technology itself. Science and technology can create opportunities. Whether and how these opportunities are exploited depends on political choice. In this sense, it can be argued that each society gets the science and technology it deserves.

These emerging social and environmental problems have lent themselves particularly well, over the past few years, to large splurges of international pontification. They have helped divert attention from other policy problems related to science and technology where severe tensions sometimes exist among nations. Everybody can agree easily in an international forum that what is needed is a cleaner environment and a better quality of life. The problems in attaining this end exist mainly *within* countries rather then *between* countries. Some new international action obviously is required: making arrangements for joint programs of ecological research and for environmental monitoring; preventing environmental regulations from becoming barriers

[38] David Hamilton, "Advanced Passenger Train Revealed," *New Scientist and Science Journal*, L (June 10, 1971), 624–25.

to international trade; stopping one country from fouling up another's environment; exchanging relevant experience and information on how to deal with the various problems.

However, although the direction of R & D efforts as well as of international relations will increasingly be affected by policy measures to deal with these "imperfections" of the market system with regard to the environment and public goods, it is far from certain that these measures will have a major impact on the pace and direction of government funding of R & D in the 1970's; or that, after the Stockholm Conference of 1972, their effects on international relations will be quite as considerable as has sometimes been claimed. Indeed, it is likely that both national R & D efforts and international relations will continue to be affected more significantly by another "imperfection" in the market system, namely, the conditions of access to enriched uranium technology and rocket launcher technology.

ENRICHED URANIUM AND ROCKET LAUNCHERS

Enriched uranium and rocket launchers are key inputs into the generation of nuclear power and the operation of applications satellites. They are also key components in a nuclear strike capability. The technologies were first developed for this latter purpose, mainly in the United States, which as a result has a virtual monopoly of both in the non-Communist world. But in spite of the fact that the U.S. Government makes both enriched uranium and rocket launching services available to foreign countries for civilian uses on a nondiscriminatory basis, a certain uneasiness persists. In sectors as sensitive to the functioning of the national economy as energy and communications, nation-states are naturally reluctant to be completely dependent on a foreign monopoly supplier, however elighted and well-intentioned he may be.

Furthermore, the Western European countries and Japan are less willing now than they were ten years ago to be satisfied simply with American assurances on supply. In the intervening period, the key importance of these two technologies for the future has become clearer, and the general desire for diminished dependence on the United States has become greater. Europe is increasingly committed to developing its own lines of nuclear power reactors, and does not wish to be irrevocably dependent on its strongest competitor for its supply of nuclear fuels. In rocket launchers, Europe does not have the same ambition to compete with the U.S.A., but some countries are very concerned about the degree to which dependence on American launchers might affect the future development of intro-European communications. In addition, Europe and Japan now have much greater technical and financial resources than they had ten years ago, so that the temptation to redevelop the technologies indigenously is now a real one. Such redevelopment would ensure security of supply, but it would be expensive. Opinions differ within and between countries on whether it would be worth the extra cost to do so. This means that what finally is done depends to a considerable degree on the position taken by the U.S. Government.

Here, a restrictive attitude toward foreign access to and control of the two technologies might at first sight appear to be most attractive to American commercial and security interests, but it may in fact

have a result precisely opposite to the intended: it could encourage those groups in Europe and Japan who support indigenous redevelopment. If these groups were successful, it would probably result in a reduction of commercial outlets for U.S. industry, and—even if the developments were undertaken for ostensibly non-military purposes—there would be a wider number of strategic options open to both Europe and Japan. On the other hand, if the U.S. Government were to take a more liberal position, giving foreign countries greater access to American technology, but only for civilian uses, some commercial outlets for U.S. industry could be maintained, and the probability of the development of independent capabilities would be reduced.[37]

CONCLUSIONS

When, during the 1960's, the United States and Soviet Russia were engaged in the space race, the U.S. Government pouring enormous and growing sums of money into university science, and American firms invading Europe's high-technology markets, it was fashionable to argue that the "imperatives of modern technology" had made the European nation-state obsolete and irrelevant, that European technology was hopelessly managed, that the scale of modern technology made European countries too small to be efficient, and that Europe must change its ways and unit politically, if it were not to be technologically depleted. Yet, now that we have statistics on trends in R & D expenditures in the 1960's we can see that the U.S.A. has been losing its preponderance in the Western World's science and technology. We can also see that countries like Sweden, Switzerland, and the Netherlands were able to run effective programs in both science and technology despite their smaller size.

Certainly, a number of countries still have difficult problems to solve in adapting to modern technology: better links between the universities and industry are still needed in France, Italy, and Japan; France has to continue to ease itself out of some expensive but unrewarding R & D programs; Germany will have to go through the same, sad learning experience in big technology as Britain and France; Canada must find effective means of encouraging industrial entrepreneurship; Italy and Japan will have to deal more effectively with the social consequences of rapid technological advance; and all European countries must decide whether or not they are willing to pay the political cost of greater specialization and interdependence that is necessary for effective programs in certain expensive technologies.

Yet it is possible to argue that by the end of the 1960's, both the small and medium-sized industrial powers of the Western World had more or less made their peace with modern technology, and that in the 1970's it will be the two superpowers that will have the greatest difficulties in doing so. In the United States, it may be hard to accept the relative decline of American scientific and technological prowess; to admit that none of the contemporary challenges—ecological or urban, German or Japanese—justify the same level and type of govern-

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[37] For indications of current thinking on these problems in the U.S. Government see statements by Pollack, Frutkin, and Kratzer in *A General Review of International Cooperation in Science and Space,* Hearings before the Subcommittee on International Cooperation in Science and Space of the Committee on Science and Astronautics, U.S. House of Representatives, 92nd Cong., First Sess., May 18, 19, and 20, 1971. (Washington, D.C. 1971).

ment support of R & D as the strong pressure to respond vigorously to a perception of a strategic and political threat by a foreign power; to generate the resources and mechanisms for the investments and regulatory activities necessary to deal with the social consequences of technological change. In the U.S.S.R. it may become increasingly difficult to resolve the conflict between a growing need to apply technology effectively in industry and agriculture as well as defense, and the upheaval in the present patterns of power and control within the U.S.S.R. that would result from creating the conditions for this to happen.

The above discussion has been concerned with only a small proportion of the world's population. Nothing has been said about the less developed countries. Some may consider this unsurprising: very little R & D is done there, and nearly all the R & D done in the advanced countries is not related to the needs of the less developed ones. The prime objective of industrial R & D is to save on already expensive labor and to respond to the rising needs of societies whose affluence is already considerable. No Western government devotes more than 2 per cent of its R & D budget directly to the problems of the less developed world; most countries devote much less.

During the 1960's, multinational firms showed a growing tendency to set up component manufacture and assembly plants in the less developed countries to take advantage of lower labor costs. In particular, the experience of the electronics industry in Southeast Asia suggests that—labor unions in the advanced countries permitting—such activities will be considerably expanded in the future: provided that the population has primary education, labor costs are considerably lower than productivity levels by comparison with the advanced countries. But although this trend could make an important contribution to meeting foreign exchange needs, its effect on the huge employment problem of the less developed countries is less striking, since the possibilities for making existing production methods more labor-intensive are strictly limited.

This is but one example for the much wider problems, namely. that the present pattern of the world's R & D is directed to making labor-*saving* production methods more efficient, although the less developed countries need more efficient labor-*using* production methods. There may be also the problem that technological progress in the advanced countries leads to substitution and economics in the use of raw materials, and thereby turns the terms of trade against the less developed world that is dependent on the export of raw materials. The present Malthusian contention about the depletion of natural resources could have the dangerous effect of accelerating such a trend. There is also the established fact that the introduction of health and sanitation technology, without concurrent action to establish birth control, has led to the population explosion in the less developed countries, and to the attendant difficulties of raising living standards.

Some scientists are shocked and surprised when it is argued that the present world pattern of R & D has been detrimental rather than helpful to the material well-being and welfare of the less developed countries.[38] They may even become offended when the disastrous effects

[38] For a convincing presentation of this argument, see Charles Cooper, "Science and the Underdeveloped Countries," *Problems of Science Policy* (OECD, Paris 1968).

of introducing health and sanitation technology are pointed out. Certainly, it was and is a noble objective to stop people from dying prematurely, but the attainment of noble objectives can have unforeseen and harmful consequences. This seems to be well recognized with regard to the environment: we are today treated to a continuous stream of homilies about how the production of goods and services can cause us to spend too much time in traffic jams and can stop us from swimming, sailing, and fishing in lakes. But these harmful effects are trivia by comparison with the net harmful effects of contemporary technology on the less developed countries. It would be naive to expect that R & D efforts directed to the needs of the less developed countries would of themselves lead to salvation, but such efforts would not do any harm and might even do some good. Only the Canadian Gvernment has so far made the promotion of such R & D an explicit objective of its national science policy.[39]

TABLE 1.—COMPARISONS OF NATIONAL R. & D. EXPENDITURES IN 1961 AND 1967 (U.S.=100)

Country	Government-financed		Industry-financed		Total		Total per capita	
	1961	1967	1961	1967	1961	1967	1961	1967
United States	100	100	100	100	100	100	100	100
France	10	19	10	16	10	18	40	71
Germany	8	13	15	22	10	17	34	55
Japan	5	10	17	22	10	15	19	30
United Kingdom	19	15	25	24	21	18	74	67

TABLE 2.—COMPARISONS OF NATIONAL R. & D. EXPENDITURES IN 1963 AND 1967
[United States=100]

Country	Government-financed		Industry-financed		Total		Total per capita	
	1963	1967	1963	1967	1963	1967	1963	1967
United States	100	100	100	100	100	100	100	100
Canada	2	3	3	4	2	4	24	34
Belgium	1	0	3	3	1	1	27	26
France	12	19	12	16	12	18	49	71
Germany	10	13	18	22	13	17	42	55
Italy	2	2	5	5	3	3	11	12
Netherlands	2	2	5	6	3	4	50	57
E.E.C. total	26	37	43	51	32	42	35	46
Austria	0	0	0	1	0	0	6	12
Norway	0	1	1	1	0	1	21	30
Sweden	2	2	4	4	2	2	61	60
United Kingdom	17	15	25	24	21	18	73	67
E.F.T.A. total [1]	20	17	29	29	24	22	63	59
Japan	6	10	18	22	11	15	21	30

[1] Switzerland, Denmark, and Portugal excluded.

Note: Sources for tables 1 to 3: Yvan Fabian, Alison Young, and others, "R. & D. in OECD Member Countries: Trends and Objectives" (Paris 1971); "International Survey of the Resources Devoted to R. & D. in 1967 by OECD Member Countries: Statistical Tables and Notes: Vol. I: Business Enterprise Sector," OECD document DAS/SPR/70.7 (Paris 1970). In compiling tables, it has been assumed that, at conventional exchachange rates, the costs of performing R. & D. in Europe are 60 percent of what they would be in the United States; in Japan, 55 percent of United States costs.

[39] Christopher Marlowe, *The Jew of Malta,* Act IV, scene 1.

TABLE 3.—COMPARISON OF INDUSTRY-FINANCED R. & D. IN 1967 (U.S.=100)

Country	Aircraft and missiles	Electrical products and instruments	Chemicals, drugs, and petroleum products	Nonelectrical machinery	Ferrous and nonferrous metals and fabricated metal products	Transportation equipment, excluding aircraft	Food and drink, textiles, rubber	Other	Total manufacturing
United States [1]	100	100	100	100	100	100	100	100	100
United States per capita	100	100	100	100	100	100	100	100	100
France [2]	14	16	17	11	11	17	23	28	16
Germany	2	30	36	20	48	28	10	10	26
Belgium	0	2	4	1	6	0	3	5	2
Italy	(3)	4	7	1	4	13	10	3	5
Netherlands [4]	(5)	[3] 14	10	(5)	(5)	(5)	13	4	7
Total EEC	15	67	75	33	69	58	57	49	56
EEC per capita	16	71	80	35	74	62	60	53	60
United Kingdom	14	30	22	24	29	22	47	43	25
Sweden	(5)	5	2	6	12	2	5	8	4
Norway	(5)	1	0	0	2	0	1	2	1
Denmark [6]	(5)	(5)	(5)	(5)	(5)	(5)	(5)	(5)	[7] 1
Switzerland [8]	(5)	(5)	[9] 13	(5)	(5)	(5)	(5)	(5)	5
Austria [1]	0	0	2	0	1	0	1	1	1
Total EFTA	14	36	39	31	44	25	53	53	37
EFTA per capita	38	95	96	83	116	67	138	142	85
Japan	(5)	27	27	15	50	23	42	44	24
Japan per capita	(5)	53	53	30	97	44	81	86	48

[1] 1966 data used for United States and Austria.
[2] Data corrected to exclude funds from government.
[3] Machinery assumed to be all electrical.
[4] Total intramural expenditures on R. & D.
[5] Indicates that the figure for the sector is included in another sector or in a total.
[6] Denmark excluded in all but total manufacturing; population of EFTA adjusted.
[7] ——— ——— ——— ———.
[8] Switzerland excluded in all but total manufacturing and chemicals and drugs; population of EFTA adjusted.
[9] Chemicals and drugs assumed to be 60 percent of total manufacturing.